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OF
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**BEING THE MEDICAL SECTION OF THE
BRITISH JOURNAL OF PSYCHOLOGY**

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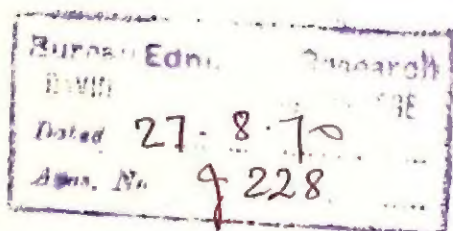
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PERCEPTION AND EGO FUNCTION*

BY T. F. MAIN

The degree of veridicality of perceptual performance has always excited the interest of psychoanalysts because it has so often provided clues about the laws of mental functioning. When little Hans (Freud, 1909), seeing a cow being milked, said that the milk was coming out of its widdler, the poor veridicality of this and of other acts of perception of the little boy led to the inference that Hans, as well as being ignorant and inexperienced, also had a particular sexual curiosity which led him to misinterpret the world around him in terms of his prevailing emotional preoccupation. It was also noted that the same curiosity led him, however, to genuine inquiry and to attention to the world around him, so that in spite of some errors—such as concluding that a railway engine must have a widdler because water flowed from it—he acquired useful knowledge.

It is possible for us to infer that his perceptual apparatus was being actively used by his ego in the service of his needs, of his early sexual interests, and, perhaps, of anxieties associated with them; that is to say that his perceptions were far from passive but were actively motivated. Secondly, we may note that his perceptions involved a particular kind of interpretation of the objects around him, animistic but 'self-ish' and related to his own body functions and the pleasures and problems associated therewith. Thirdly, we may note that a simple logic of *pars pro toto* was in operation, and that his recognition of external reality—his veridicality—was under the influence of the primary processes and the pleasure principle.

It is already clear from this example alone that perception can be typical for the subject,

* Read at the Annual General Meeting of the British Psychological Society, St Andrews, 31 March 1957.

and that the study of perception in relation to personality, nowadays a going concern in psychology, is necessarily of prime importance in the field of psychoanalysis. The example is of course far from being typical for perception in a mature individual from whom we would expect better veridicality than from little Hans. The adult ego is more mature, its use of defences such as projection, reversal, denial, isolation, identification, etc., to protect itself from impulses from within, is much more skilled, and it is better able to contain and transmute and defer the discharge of inner tensions, better able to attend to and judge reality. The techniques and functions of the adult ego are more numerous, and the basic ego function of perception is better defended against colouring by primitive anxieties and interests. But this is not to say that even in maturity it is ever free from these. Moreover, everyday work in clinical psychoanalysis shows how much perception is coloured, not only by instinctual needs, but also by the defensive processes erected against them. The experiments of psychologists like Frenkel-Brunswik (1951) confirm the conclusion that there are different perceptive modes or styles which seem to be related to the types of defence mechanisms used by an individual. We know that perception is much decided by the emotional significance of the object for the subject, by prevailing bodily needs, by prevailing moods, and—I would add confidently—by the immediate interpersonal context (a matter all too often overlooked by experimenters). An isolated sense impression cannot therefore be looked on as a simple affair, for it is the complex feat of a well-developed ego, and it requires a great deal of mental work, including a selection from and a disregarding or denial of other percepts from both the inner and outer worlds. It is an act

of total controlled behaviour from a sensory-motor-vegetative unit which has matured.

In their daily work psychoanalysts use the important principle of genetic continuity, and it is their particular concern to view mental acts not as arising *sui generis*, but as part of a developing series. The genesis of the ego, the part of the mental apparatus concerned with perception, is therefore a matter for detailed study by them, using clinical evidence from the analysis of adults and children, as well as observational data. Such study for the past thirty years has shed much light on the early development of the ego, but we must look much to the experimental psychologist, working particularly with infants and young children, for aid in finding and verifying and checking facts and exploring and testing hypotheses.

Psychoanalysts' findings indicate firmly that to begin with there is no dichotomy of body and mind. The ego is first and foremost a body ego (Freud 1923), ultimately derived from bodily sensations which in themselves are ill-located in a body the very boundaries of which are not clearly experienced by the infant. Early perception is nearly exclusively proprioceptive. Actions and reactions are at first purposeless, although primitively patterned and do not take place in response to outer stimulation unless this is strong, for the infant has a high perceptive threshold, a stimulus barrier which is perhaps a bodily forerunner of the mental act of repression. The early mouth activity of infants begins, not with the perception of food, but with proprioceptor perceptions; and lacking proprioceptor stimuli the exteroceptor perception of food does not occur. With external help, food slowly becomes related to proprioceptor perceptions and then later at about two months the infant becomes able to perceive food outside the hunger situation (Spitz, 1953). It has to be noted that even at this age early experience of objects other than the self, is nevertheless 'self-ish', and is tied to the fact that such objects can provide a modification of *proprioceptive* unpleasure by allowing the discharge of tension derived from bodily sources.

At first primitive perception is always associated with motion, of massive but localized order. The basic type of experience is probably synaesthetic, and even in adulthood synaesthesia remains one of the basic principles of perception (Schilder, 1942). The integrating functions of the ego are a late development indeed, and the development of differentiating functions precedes it. Similar to the way in which embryos give undifferentiated massive responses to any effective stimulation, the earliest period is an undifferentiated one in which experiences appear to be diffuse and total, involving motor action and, according to Schilder, all sense modalities although in varying degree. Primitive perception is part of *total* reaction involving pain or pleasure to a bodily situation accompanied by movement.

In terms of mental structure Hartmann, Kris & Loewenstein (1946) describe the earliest phase as undifferentiated, a common matrix of inborn endowment from which both ego and id arise by a process of differentiation whereby the perceptual apparatus is eventually integrated into the ego. The ego, described by Freud as being in the closest contact with the id, has perceptive functions related to both inner and outer reality, but in its early phases especially it is liable to be invaded overwhelmingly by impulses from the id, the reservoir of instincts; nevertheless, it is capable of experiencing, in terms of pleasure-unpleasure, and of discharging tension by fairly diffuse motor action in the interests of homeostasis. The earliest origins of the ego and particularly of its function of perception are not, however, clearly understood.

Glover makes an attractive proposal, tying the early ego to proprioceptive disturbances and to their cessation in conjunction with external sources of help. In his view any system which can represent a positive libidinal relation to objects, which can discharge reactive tension against objects, and which can in one way or other of these ways discharge anxiety, is an ego-nucleus. An oral ego-nucleus is primary, but ego-nuclei related to other zones also form. It is to be noted that

his view concerns the representation of id-drives, and very significantly contains object relations, albeit of a primitive kind with part objects. This theory of ego origins does not, however, grant us knowledge about the beginnings of perception, for primitive perception is implicit in such object relations.

Hartmann (1939) propounds a theory, which solves a number of difficulties, that the ego has one of its roots in somatic and psychic apparatuses which are free from psychic conflict, and of the psychic apparatuses he declares the probability that perception is one; that it is not acquired but is built on constitutional endowment, and that it is primarily autonomous. (He of course accepts that perception may at various times become involved in conflict, and therefore that the primary processes operating as they do, not with the reality principle but with the pleasure principle, may disturb severely the veridicality of perception in such ways as condensation, *pars pro toto*, equation of opposites, etc., together with ego defences such as rationalization, projection, etc.) His particular contribution also includes the further notion of secondary autonomy; that certain ego-functions developed as an outcome of defence against id drives are an outcome of defence against id drives are not forever thereafter maintained by conflict but may later become more or less independent of threat by id-drives and so secondarily become conflict-free. I hope it is plain that his concepts in no way absolve any scientist from noting the forces that *motivate* perception, and that unremittingly select or suppress the fruits of perception and use them for unconscious ends. His point is that the psychic apparatus of perception may from the very first be innocent of the id; the innocence of the user of the apparatus is not however to be presumed. His ideas pave the way for psychoanalytic interest in conflict-free secondary process thought, so assiduously studied by academic psychologists, but about the origins of perception they do not go beyond Freud's formulation that the system perception-conscious which is part of the ego has the prime function of perception.

Fairbairn's unified psychoanalytic theory of dynamic structure postulates an early ego energized by instinctual forces and related to objects from the first, split into three dynamic structures, two of which, together with their objects, are then repressed. His reformulation of Freud's three systems of Id, ego and super-ego explains how the early ego and its functions, like perception, achieve a precarious measure of reality-orientated activity in the face of threats from internal forces and structures of an instinctual and censoring order. His theory, in common with Glover's and Melanie Klein's on early ego development, does not emphasize concepts of autonomic function and maturation of perception, but is concerned, like the latter's, rather with the nature and development of early object relations and their significance for mental (including ego-) development. All of these theories rely, however, on the idea that perception of objects begins with instinct-driven proprioceptor perceptions, internal needs experienced in the body meeting (and failing to meet) objects with the capacity more or less to relieve the tension experienced. Because of the primacy of proprioception these objects are experienced at first as parts of the self, and are therefore endowed by the individual with good or bad qualities of a sensory proprioceptive character; that is to say, in early perception the ego does not passively await an imprint from the external world but brings to the act of perception much of itself and its instinctual contents.

It would seem that in the earliest feeding relationship* the infant is enjoying not the nipple and the breast but rather its mouth and itself in a somewhat undifferentiated synaesthetic way *under conditions which permit it to*. These conditions are the early objects, and the infant's dependence on them for alterations in its proprioceptive tensions could be described as the early object relation. With

* The feeding relationship is chosen only as an illustration. Its importance is beyond question and it is at present the best studied, but it is not the only early relationship.

subsequent development the bodily parts and sensations become differentiated and the objects become increasingly experienced as external agents, but at first much endowed with properties that have their origin in the internal world of proprioception; felt to be not so much a *part* of the self but still as *belonging* to the self. In these early stages perception is largely instinct-driven, tied to the body needs and their differentiation and to the special conditions which objects provide for their satisfaction. Thereafter objects are slowly differentiated as having animistic independence in the external world.

None of these views on the nature of primitive perception and the early ego allows us to postulate an ultimate genesis of perception. We have to be content with observing that man is born with certain primitive ego dispositions, and that the capacity for diffuse undifferentiated proprioceptive perception is inherent in the apparatus with which he is endowed.

Psychoanalysis has, however, demonstrated that perception as well as other ego functions is also the agent of the id and eventually of the superego in degrees unimagined only half a century ago. The case of little Hans alone is unforgettable, and we cannot leave to one side its implications in any view of perceptual or ego maturation. In this connexion I must draw attention to the fact that mental maturation is a concept which tends to have the flavour of a value judgement; perhaps we should remind ourselves frequently that it is simply a particular mode and direction of development, of maximal differentiation of behaviour, and that other involved, lengthy complex dynamic developments (such as schizophrenia) are also 'natural'. It may be that we tend to regard maturation as being more 'natural' because as human beings we prefer that particular kind of development. At all events, *while remembering intrinsic development tendencies*, we have to remember that maturation is far from being automatic; it is always the outcome not of a lack of interference with 'natural' developments but of a

very special set of environmental circumstances (or 'interferences') without which very different developments may occur. Hartmann requires what he calls an 'average expectable environment' for the growth of the ego adaptations he describes, but in truth he thereby assumes a highly specialized, active, complicated, adaptive situation containing very much mothering—feeding, warming, cooling, holding, stroking, jiggling, patting, teaching, eye-catching, rewarding, stimulating, soothing, etc., as well as calculated and inevitable inactivity and frustrations, the whole situation being of a kind that sufficiently complements the infant's internal needs as they arise. We know that given perverted mothering (Spitz, 1945, Bourne, 1955) different infant adaptations are made, some positive, but others negative, the latter including (from the point of view of maturation) failure to sit up, to stand, to crawl, to walk, or to develop the ordinary social responses, extending to amentia and the grossest limitations of perception.

For normal development of the infant—indeed for its very life—it is essential that at first there be sufficiently frequent, regular, careful perception of his needs by *another*. Before the infant can differentiate and accurately perceive his needs, this other *must* estimate these needs, their multiplicity, intensity, bodily locations and quality. Then the seeking, finding and relating of appropriate objects to the infant in accordance with these needs is in the first place the task of this other if the infant is to live and mature. Biologically, there is no such being as an infant; there is only an infant and its mother.* By selecting the infant for discussion, as we sometimes must, as if it were a complete organism in its own right, we run the danger of discussing an abstraction, and of ignoring the host of special conditions that have to be observed by the environment if there is to be an infant to discuss, and of regarding as automatic or autonomous

* This remark is borrowed from D. W. Winnicott. He does not remember his making it, but I do!

developments which are highly conditional. The ethologists (e.g. Lorenz, 1935) in our lifetime have shown that certain skills and behaviour patterns of animals once regarded as simply inborn and autonomous in their development require appropriate conditions from the environment for their development, and though for reasons I will discuss later it may be inappropriate to apply to the study of human behaviour findings obtained by animal observation and experiment, at least these observations with animals must make us cautious about accepting any view that functions of the ego, including perception, are automatic in development. However, even to regard the function of the perfect environment only as actively to guard and to evoke in-built potential for development is to ignore some well-confirmed analytic findings about the mechanics of ego-growth.

Freud (1923) pointed out that the ego is more than merely that part of the id which is modified by the influence of the perceptual system, more than merely the representation in the mind of the real external world. He declared (1915) that in so far as the objects are sources of pleasure they are absorbed by the ego into itself, introjected, to use Ferenczi's term, so that the ego assumes the features of the object. If it is recalled that the object itself is perceived in the first place in terms of proprioceptor perceptions, and as part of the self, and that it is endowed by the infant with the capacity to provide perceptions of a pleasurable or pain-relieving kind, we can follow that the ego is enriched by the process of introjection in a unique way. The ego takes into and unto itself that which it experienced in the relationship—the mediation of tension—originally a property of what to an observer would seem to be an external object. In this country Melanie Klein and her co-workers (e.g. Paula Heimann, 1952) lay special emphasis on the functions of introjection and projection as the instruments for the very formation of the ego, on the inestimable share these mechanisms have, in modifying the id into the ego, by adding something new to the self,

or ridding it of something of its own, and on the way the ego function of perception is intimately bound up with the id-driven ego-activities of introjection and projection. These ego mechanisms do not of course arise *sui generis*—they are patterned according to primitive judgement—'I like this', or 'I don't like this', to use an adult way of expressing it. For Freud the id is the mental representation of the bodily instincts, and he (Freud, 1925) described these early ego judgements of good or bad in terms of oral id-impulses—'I should like to eat that, or I should like to spit it out.'

Following early introjections (which, we recall, are of objects related to bodily needs), hallucinatory satisfactions with the internalized object can for a time be used to deal with rising needs. The inevitable failure of hallucinatory gratification leads, however, to the need to discover the object afresh, and in an active relationship with the mother, the infant does so. By this process, frequently repeated, the inner world of fairly stable introjected objects is slowly built up, the difference between inner and outer objects is slowly established, and the beginnings of the adjustment to reality are made in the early ego.

The process of projection and introjection in the *infant* which are inferred from clinical work are of course easily observable in the *child*. The little boy who babbles tenderly to himself as he soaps himself in his bath does so because he has taken into himself his tender soaping mother. The lonely little girl who comforts her doll has projected her loneliness into it and is identified with her own introjected mother. It may be noted here that such behaviour is not merely characteristic of the children in question; it is related somewhat to the mothering they experienced.

Work by Coleman, Kris & Provence (1953) on types of mothering shows how fateful for the infant's ego functions is her capacity to respond; and Anna Freud has drawn attention to the prominence of mouth activity as a general response to a variety of bodily needs in those children whose mothers had themselves only the one response of giving suck

when their children were in states of painful tension.

If, however, we leave object relations and turn for a moment to that unviable abstraction, the single infant divorced from its environment, we cannot but be struck by the fact that it is so helpless for so much longer a time than other mammals. Infants at birth (Fries & Woolf, 1948) vary considerably and fatefully in their inborn capacity for activity, and many psychoanalysts have concluded from their clinical work alone that there are considerable constitutional differences between individuals in the instinctual endowment, and in the relative strengths of the aggressive instincts and libido; but even the most active neonate is peculiarly ill-equipped for survival except in the specialized circumstances of years of maternal care. Even the capacity to suck seems not to be reliably inherited in mature form. It is true that one may find a rare baby born with macerated fingers and it may be that it has actually been thumb sucking in utero perhaps by the (chance?) association of hand and mouth evoking early sucking responses of the kind that stimuli can elicit in embryos (Gesell, 1945). But a more striking fact is reported by Ribble (1943), that of 600 infants, at least 40% had to be taught to suck, by having their mouths opened by another, the nipple inserted well inside their mouths, and the chin worked rhythmically up and down. Although there does seem to be something like an inherited sucking response which can with more or less difficulty be established in all babies, it seems that for many their mouths have to be located for them and worked *by another* before they can themselves begin reliably to differentiate mouth sensations out of the general synaesthetic distress.

It is common observation that the infant needs other undifferentiated bodily problems to be differentiated by the mother in the first place, the body position required for comfort, freedom to breathe, freedom from noise, light, for proper temperature and so on. Ribble has described the interlocking requirements to be held, to be stroked and to have body con-

tacts, if good sucking and even breathing are to be established, and we know of the great importance to the later growth of the ego of being stimulated at the proper pace, in proper amount, of being dandled, cooed at, having the mother within reach of the eye, being sung to, having coloured toys waved before the eyes and so on. The point I am making is simply that the 'average' infant not only requires but gets the mother in many varied aspects for many hours a day, in her role as perceiver, diagnostician and therapist, or if you prefer the analytic term, as the interpreter, as well as the provider of its needs and stimulator of its potential, almost as frequently as it experiences major crises of ill-differentiated id impulses. The introjections of this early period, upon which the ego feeds, and which contribute so much to its character, are aspects of the *mother who becomes aware of*, diagnoses, and treats proprioceptive tensions and exteroceptor situations and who experiments with its potential out of identification with her child. The growing *awareness in the infant* of the locale, nature and intensity of bodily needs out of a fairly diffuse synaesthesia and of its capacity for exploring its potential depends not merely on the maturation of the nervous system (as the instances of functional failure mentioned above clearly show) but on the introjection of the complementary awareness, diagnoses, and activities *of the mother*. Aided by these introjections the ego comes to feel, localize and act on its own needs. The child's capacity to attend to itself and the world, to perceive and diagnose its own needs and distresses, their locale and nature and intensity, and to perceive the objects of its requirements, has indeed a precursor and stimulator in the mother. The perceptive mother not only aids the growth of the ego but of perception itself. It may even be that the very genesis of perception in a human being is conditional—I would put it no higher—on appropriate introjections of the perceptive abilities of another.

In working towards this idea I have by no means even sketched the general contribution

of and the problems raised by psychoanalysis in the understanding of perception and ego-growth. I have omitted, for instance, all mention of libidinal development, of the interplay between phantasy and bodily needs, and of the important part played by early phantasy in the way internal and external objects are regarded and dealt with by the infant. And I have not even mentioned the great part aggressive forces play in the processes I have outlined. I must not omit, however, some explanation of why I have hesitated, in this contribution towards a unified developmental psychology, to draw on some observations and experiments which show by contrast how ego-nuclei and perceptual functions can be well developed and ready for autonomic function at or shortly after birth in most animals. Partly this is because comparison between species is apt to be misleading, but partly because of evidence that the helplessness at birth and the long period of dependence on a specialized responding environment, which characterizes the human being, are singular features with consequences that need study in their own right. In matters of psychology the proper study of mankind may indeed be man, because of the findings in the field of comparative anatomy (Bolk, 1926) that suggest strongly that man is a neotenic mammal, unlike the others. According to these findings man is not sprung off a mature parent stem, but rather

from a foetal creature of the anthropoid series which developed genitalia; a creature of the same order of development as the axolotl, an arrested larval form of salamander which, because of disturbances in the temperature in the water, stops developing, grows genitalia, and takes to breeding its own kind in its own right. This may explain the long period of helplessness of the human infant and the relatively unorganized and undifferentiated foetal state of the bodily activities, the undeveloped ego and frail ego-nuclei, which characterize him at birth and which distinguish him from all other animals. But whatever the explanation, these singular features leave him much longer at the mercy of his general environment and for better or worse, much more dependent on, and therefore much more enriched by, the processes of introjection and projection.

I would therefore emphasize that—more than any other—human babies are very unformed, uncompleted creatures with few capacities and that maturation is quite conditional upon and inseparable from their nurture; and that the internalization of the perceptive skills of the mother during the lengthy period of nurturance plays a vital role in the genesis and growth of the ego's function of perception. Lastly, I hope it is obvious that throughout the word mother is meant to represent he, she, or those who do the mothering.

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ASPECTS OF PERCEPTION IN PSYCHOANALYSIS AND EXPERIMENTAL PSYCHOLOGY*

By THOMAS FREEMAN

When psychoanalysts have turned their attention to perception they have always considered it in relation to the remainder of mental activity. They have been concerned with demonstrating the connexion between disturbances of perception and unconscious mental processes. Freud (1910) pointed out, in the case of vision, that abnormalities of this form of perception will arise when it becomes the site of unconscious conflict between the sexual drives and the forces of repression. This dynamic interpretation of perception as being constantly influenced by unconscious wishes, prohibitions and anxieties is the best known. It is, however, only one facet of the psychoanalytic theory of perception. A comprehensive description of a mental process, from the psychoanalytical standpoint, must include the topographic and economic aspects as well as the dynamic. Such a description is only possible in terms of the concept of the mental apparatus (Freud, 1900).

The mental apparatus was conceived of by Freud (1900) as an entirely psychological construct. It was not tied to anatomical sites or physiological substrates. Emphasis was placed upon function rather than on structure. Mental activity was envisaged as the outcome of a dynamic interaction between the systems of the apparatus. The degree of intensity of the processes involved in the dynamic equilibrium to which the apparatus tended is referred to as the economic aspect of mental activity. The homeostatic balance of the apparatus was thought of as being constantly disturbed by the demands of bodily needs and external stimulation. In the fully developed state two

distinct types of mentation were to be found—the primary and the secondary processes. Topographically the primary process was located in the system Unconscious and the secondary process in the systems Perceptual-conscious and Preconscious. The primary process, through condensation and displacement, sought the reduction of accumulated excitation by immediate discharge. At this primitive level perception is closely tied to the satisfaction of instinctual needs—the heightening of excitation leading to a greater awareness of bodily sensations. Failure of discharge results in the hallucinatory revival of memory traces of previous gratification, e.g. of sucking at the breast. The secondary process, integral to the system Preconscious, utilizes memory, perception, motility and the capacity to bind excitation (i.e. to accept delay) to reduce tension by means of altering the environment and thus to bring about suitable circumstances for need satisfaction. The prototype of thought is represented in the system Unconscious by the drive-orientated memories of previous instinctual gratification. In the system Preconscious, ideation has freed itself from the immediate influence of the instinctual drives. Thinking is verbal and logical. The harmonious integration of the primary and secondary processes results in thinking which is both reality adapted and at the service of the bodily needs.

Perception of both external and internal stimuli takes place by way of the system Perceptual-conscious. This system is situated at the periphery of the apparatus and impinges on the physical environment. It is protected from overstimulation externally by a defensive barrier. Behind this lies the perceptual surface of the system. In contrast to

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the other systems of the apparatus, where stimulation results in an enduring record, no permanent trace of impressions is left in the perceptual apparatus. If the system was unable to erase perceptions once they had been registered it would lose its capacity to receive fresh stimulation. Adjacent memory systems provide a permanent record of perceptual experience. Clinical phenomena underwrite this close connexion between perception and memory. As in the case of external perceptions, internal sensations and feelings only become conscious through reaching the system Perceptual-conscious. If their entry is barred they remain unconscious—as unconscious affects. Such a conception is justified in that it maintains the analogy with unconscious ideas. Freud (1923) has proposed that it is the addition of preconscious verbal images to the processes which occur in the depths of the mental apparatus which enables thinking to become the object of perception. As the memory traces of words originate from auditory perceptions the role of this form of perception assumes a great significance for the development of secondary process thinking.

The psychoanalytic contention that the essence of what is mental is itself unconscious has implications of the greatest importance for perceptual theory. Perception is the end result of a series of excitatory processes of a dynamic and economic type, occurring within the mental apparatus. Freud (1925) has proposed that consciousness and thus the capacity to perceive is dependent upon the system Perceptual-conscious being 'innervated' by energetic processes (i.e. cathected) from within. As long as the system is activated it can receive perceptions, but once the cathexis is withdrawn consciousness is extinguished. Freud (1925) says: 'It is as though the unconscious stretches out feelers, through the medium of the system Perceptual-conscious, towards the external world and hastily withdraws them as soon as they have sampled the excitation coming from it.'

The introduction of the structural approach—ego, id and super-ego (Freud, 1923)

—did not imply any basic alteration in the hypothesis of the mental apparatus. The ego was defined as '... a coherent organization of mental processes...' (Freud, 1923) in contrast to the id. Its matrix was the sensations emanating from the body and especially from the body surfaces. The ego is essentially a body ego. The systems Perceptual-conscious and Pre-conscious belong to the ego and their functions now became ego functions. The ego concept enabled psychoanalysis to integrate the clinical observation of unconscious resistance into their theoretical framework. Much of the ego is unconscious in the dynamic sense and gives rise to the inevitable resistances encountered during psychoanalytical treatment.

In more recent years Hartmann (1939) has proposed that a number of ego functions, such as perception, control of motility and memory, have autonomous roots within the ego. He has suggested that initially such functions do not arise out of a conflict situation, and are independent of the bodily needs. They follow their own laws of maturation and only secondarily become involved in conflict between the instinctual drives and the demands of the environment. This hypothesis of autonomous development of certain ego functions follows a similar theoretical view advanced by Schilder. With regard to perception he stated '... though affects and drives play a most significant role in perception, perception has a core independent of drive dynamics' (1924).

Psychoanalysis points to the unconscious ego as the site of the processes which eventually result in the perception of internal and external sensations. This emphasis upon the idea of perception as an end product leads to an awareness of the influence which other mental processes must have upon perception. Clinical experience indicates that perception can be markedly affected by the state of the other ego functions, such as those of memory, motor activity and reality testing. It is hardly necessary to mention how the bodily needs can alter perception in abnormal mental

conditions and in states of bodily deprivation. Recent studies of perceptual processes within the psychological laboratory have produced results which are of the greatest interest to psychoanalysts.

In a series of experiments Werner (1952) and his colleagues have shown that there is an interaction between sensory and tonic (e.g. kinaesthetic) elements in the formation of a perception. They have demonstrated that perception of the vertical is influenced by a number of factors. For example, an electrical stimulus applied to the neck muscles or an auditory stimulus results in the vertical being seen as tilted towards the side opposite to that of stimulation. Such experiments, they suggest, indicate the equivalence of sensory (auditory) and tonic (direct muscular) stimulation. Both affect the individual similarly resulting in identical perceptual changes. In support of their hypothesis, they have devised experiments which indicate that motor activity has a profound influence on perception. Subjects who are immobilized by being strapped in a chair while looking at a stationary point of light in a dark room have a greater tendency to see the light as moving (auto-kinetic phenomenon) than do subjects who are moving their arms continuously while making the observations.

At any moment, according to Werner's hypothesis (1952), there exists within the individual a sensori-tonic field which is maintained by an underlying dynamic-energetic process. The attributes of a perception will depend upon how the stimuli arising from the object influence the sensori-tonic state. This concept of an energetic-dynamic system with emphasis upon the intimate tie between motor and sensory activity has much in common with Glover's (1950) description of what he terms the 'primary functional phase' of the mental apparatus. He states '...the mental apparatus is (at this stage)... a series of dynamic (energetic) sequences representing and recording the flow and ebb of psychic excitation.'

While the advocates of the sensori-tonic

theory have been demonstrating the effect of somatic and visceral stimuli upon percept formation, other psychologists have been turning their attention to the part played by affective and conative tendencies. A number of ingenious experiments have been devised to test out such possibilities. Of particular interest to psychoanalysts are the experiments which deal with the role of the bodily needs and those which investigate the effect of emotionally disturbing stimulus objects. The perceptual response of subjects deprived of food or water indicate that there is a close relationship between bodily needs and perceived dimensions or intensity of illumination (see Gilchrist & Nesberg 1952). Beams & Thompson (1952) have shown that children will overestimate the size of liked in contrast to disliked food. These and other experiments of a similar type provide proof not only of the influence of the needs but also of the part which past experience plays in perception.

To examine the effect upon perception of emotionally disturbing stimuli, McGinnies (1949) presented a series of eighteen words tachistoscopically to a number of subjects. Seven of the words were of an unpleasant or crudely sexual nature. Initially the words were exposed for only a hundredth of a second but the exposure was gradually increased until correct recognition was achieved. After each exposure the subject was asked what he thought the word was. Galvanic skin responses were recorded simultaneously. McGinnies found that duration thresholds for recognition of the tabooed words were longer than for the others. He also noted that there was a significantly greater skin response to the 'unrecognized' tabooed words than to the 'unrecognized' neutral ones. McCleary & Lazarus (1950) repeated this experiment utilizing nonsense syllables. An unpleasant affect was associated with some of the syllables by accompanying a brief exposure with an electric shock. It was found that galvanic skin responses were greater for the previously shocked syllables than for those

that had not been accompanied by a shock. These and other experiments confirm the existence of a process of preconscious perception.

The experimental work quoted gives support to the psychoanalytic belief that perception, like any other mental process, first goes through an unconscious phase. There is a complex interaction, proceeding unconsciously, between bodily needs, external stimulation, internal sensations and the influence of past experience. All the evidence suggests that the individual can no longer be looked upon as a being who is stimulated, then perceives or recognizes and finally reacts. It is more in accordance with clinical and experimental experience to believe that stimulation is followed by reaction and that a part of the reaction appears as a conscious perception.

The recognition by psychoanalysts that the perceptual apparatus has an autonomous development within the ego permits a *rapprochement* with psychological theories which lay stress upon the formal aspects of perception. This development has given ample recognition to Schilder's (1924) assertion that the percept is the end product of processes, physiological in nature and unconscious in the organic sense. Nevertheless, psychoanalysis can make a further contribution to perceptual theory by directing attention to the influence of the primary process upon the perceptual apparatus. Fisher (1956) has pointed out, on the basis of dream and imagery studies, that during the physiological maturation of the ego apparatuses of perception and memory '...there are early phases during which they are capable of functioning only in certain ways, e.g. with condensation and displace-

ments. These early mechanisms do not disappear with further development of the organism but are simply overlaid by more, mature, reality-orientated perceptual and memory processes. Furthermore these early phases persist in the organism and constitute preliminary stages in the development of reality-orientated percepts and memories.'

It would be a regrettable omission if a reference was not made to the disturbances of perception which occur in abnormal mental states. Clinical observation in such conditions provides further information regarding the ways in which perception and the other ego functions interact. The normal relationship between perception and memory is so altered in many cases of schizophrenia that memories reappear as perceptions, i.e. as hallucinations. Such abnormal perceptions are often closely linked to the instinctual drives. This association between needs, memories and perception is reminiscent of some of the experiments described above. Again, it is possibly too easy to make an analogy between those experiments dealing with the influence of motility on perception and the clinical observation that in catatonia, movement can influence the patient's perception of objects. Finally it is difficult not to draw comparisons between the experiments undertaken by McGinnies and the psycho-analytic concept of denial.

In this short paper I have tried to show that the study of perception offers a meeting ground for psychoanalysis and experimental psychology. Freud's metapsychological approach, which lays stress upon the dynamic-energetic aspects of mental processes as well as the structural, offers a suitable framework for the examination of psychoanalytic and psychological hypotheses.

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PSYCHOTHERAPEUTIC ASPECTS OF MALE HOMOSEXUALITY

By L. H. RUBINSTEIN*

In a recent lecture on Psychotherapy (1956) Prof. Ferguson Rodger singled out phobias and homosexuality as generally offering a poor prognosis. Edward Glover, in his handbook on *Psycho-Analysis* (1949), states that 'an isolated phobia in an otherwise apparently fairly stable person is usually difficult to resolve'. Many cases of homosexuality can be accurately described as such monosymptomatic phobias, in which peace of mind is maintained so long as the danger situation, namely sexual contact with women, is avoided.

In most cases of homosexuality one is confronted with the added difficulty that the patients are genuinely unaware of their fears. Whereas the ordinary phobic patient knows that he is suffering from specific fears, no matter how effectively he may deny or belittle their importance, the homosexual complains—if at all—only of feeling sexually indifferent to women. The unconscious anxieties are effectively hidden by an attitude of superiority or hate in some cases, and in others by an exceptional ease in social contacts with women, varying from friendly identification to marked ingratiating. It is, moreover, all too easy for the homosexual to claim that his condition is constitutionally determined, that it is a biological variant without any psychological cause. It is not my intention to deny the presence of constitutional factors, which may in certain cases be the more important ones. Indeed, psychopathologists have never ceased allowing for constitutional factors in all forms of neurosis and character disorder. What is to be deprecated, however, is the still

prevalent tendency to assume that most cases of homosexuality can be satisfactorily explained by reference to constitution and endocrinology. One can well understand that homosexuals themselves favour this aetiology in self-defence, in order to counter-balance social disapproval as well as to maintain a façade of psychological normality. But it still happens frequently that homosexuals who are anxious to embark on a course of psychotherapeutic investigation are severely discouraged by their medical advisers, who were themselves taught that homosexuality is totally incurable. On the other hand, we have to guard against a swing of the pendulum of medical and public opinion to the opposite extreme of over-optimism. Apart from being scientifically unsound, this could only lead to the swamping of psychiatric clinics with requests to treat unsuitable cases; and it would have an unfortunate effect on the patients who failed to respond to psychotherapy if they were therefore to be regarded as wilfully incorrigible.

It follows that psychotherapy should, in the first place, be offered to those homosexuals who are already aware of some psychological disturbance, that is to the more obviously neurotic type of homosexual. In this group it is not always possible to distinguish at first whether their conscious anxiety and guilt spring from an inner conflict, or whether they are mainly the consequence of social and legal pressures. Even in cases who seem to present themselves merely on account of a court order, or because of their expectation of impending trouble, a thorough psychological investigation is indicated before they can be considered unsuitable.

In the majority of cases, particularly in view of the limited facilities for dynamic psychotherapy, the therapist has to content himself with an exploration of the more obvious causal

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and contributory factors, in the course of which the patient may obtain intellectual insight and a better understanding of his condition. Such a limited achievement should not be discounted as purely 'didactic'. It can have considerable value by helping the homosexual to feel less of a 'freak', more in line with normal development, thereby reducing some of his irrational guilt and anxiety. A more rational understanding and acceptance of himself can often benefit society as well as the individual. A large percentage of men who cause offence by homosexual behaviour in public places are those who are so ashamed of their homosexuality that they cannot reveal it to friends, but only to strangers whom they despise, more or less in the same way as they despise themselves. The opposite attitude, of those who flaunt their homosexuality and advocate it as a 'higher quality' (which causes much public indignation), is a very obvious defensive reaction designed to compensate for a deeper sense of inferiority and shame.

It will thus be seen that psychotherapy aimed at understanding and adjustment is not just a means of 'turning an unhappy homosexual into a happy one'. The idea of the 'happy homosexual' is very much of a myth, at any rate in relation to the majority of homosexuals who, with increasing age, begin to feel despair at the prospect of more or less permanent loneliness and isolation. There seem to be relatively few who are able to maintain lasting attachments. A somewhat larger number are capable of satisfactory sublimations, sometimes of a very high order. It is, however, one of many misconceptions in this field that homosexuality is largely confined to the upper strata of society, learning and the arts.

In this connexion, I should mention therapeutic attempts which mainly aim at helping the patient to achieve sublimation, in some cases by religious conversion. While one may well sympathize with these aims, one cannot overlook the fact that in many cases homosexual activity occurs side by side with valuable cultural pursuits, and indeed with a genuinely

religious outlook. As psychologists we cannot forget the distinction between substitute satisfactions of a higher order and true sublimations. Psychotherapy can at best clear the way for adequate spontaneous sublimation by strengthening the ego and by diminishing inhibitions and repressions which bar creative expression and genuine love. This difficult task is doomed to failure if the psychotherapist starts with a moralizing approach which can only strengthen the patient's infantile super-ego.

Turning to the possibilities of more radical treatment by psychoanalysis or psychoanalytic psychotherapy, the most pressing question is also the most difficult one to answer: namely, what types of patient offer sufficient prospect of improvement to justify the use of resources that are still severely limited?

The first consideration in this respect is that of age. With homosexuality perhaps even more than with other personality disorders it is obviously better to tackle the problem in its early stages rather than later when the condition has become confirmed. However, the danger signs are not usually noticed early enough to allow for treatment in childhood. And it is rare for an adolescent to be sufficiently aware of, and worried about, the ultimate consequences of his homosexual tendencies to co-operate fully with intensive psychotherapy. The general difficulties of treatment in adolescence are considerably greater where the symptom offers immediate satisfaction and sometimes secondary gain. With young people in particular, the openly neurotic person is much more amenable to treatment than the unconcerned one, who is bound to feel resentful and suspicious of being forced to give up a known gratification for one that is unknown and in addition feared. At this stage of development it is not always possible to distinguish the really serious case from the transient one of the 'late developer' type. With the majority of teen-agers it is advisable to start with a more general exploration, and to reserve continued and

intensive treatment for those who turn out to require it most and prove to be sufficiently accessible.

On the whole, the age group between twenty and thirty is the more amenable one; but one should guard against being too rigidly guided by considerations of chronological age, especially in a condition which so often goes hand in hand with general personality immaturity. If flexibility is maintained, very good results can occasionally be obtained with homosexuals between the ages of thirty and forty. I should add, however, that such rather unexpected recoveries are mostly confined to treatment by full psychoanalysis.

It is equally important not to be rigid in selection according to the type of presenting homosexual behaviour. The usual expectation that it will be easier to help an active homosexual to exchange his male object for a female one, than for a passive homosexual to change both object and direction of his sexuality is not always confirmed by experience. In the first place, a large number of homosexuals are almost exclusively attracted to mutual masturbation and general body contact wherein activity and passivity are closely intermingled. Secondly, one should always keep in mind the fundamental bipolarity of mental attitudes, especially the unconscious ones. This applies in particular to such attributes as 'activity-passivity' and 'masculinity-femininity'. Psychoanalytic experience has shown clearly how the so-called active homosexual can have a dual identification, one with his more passive, weaker or younger partner who represents his childish self, the other one with the bigger and stronger parent-figure whose part he plays. Similarly, the passive homosexual often aims at identity with the 'strong man' whose penis he incorporates at least temporarily.

This leads to the very important prognostic criterion of the strength and depth of the homosexual's feminine identification. The homosexual who genuinely wishes to be a woman and who has never accepted the idea of growing up to be a man is obviously less likely to respond to psychotherapy, even if his

condition is not at all organically determined. In my experience at least, this type of passive homosexuality is not very frequent. In the majority of cases this wish to be a woman is literally skin-deep. It arises from the fact that as girls they would not meet obstacles in the way of being loved by a man; from their narcissistic envy of women for the greater latitude allowed to them in our civilization to wear beautiful clothes and to expose more of their body; and more generally from the notion that a woman's life is more passive, easier and freer from responsibilities. When one probes this wish a little more deeply, one usually finds a very important reservation—most of them do certainly not want to part with their male genitals. Only very rarely does one come across a genuine desire for a surgical change of sex. Most passive homosexuals who feel the wish to be a woman want to be a woman with a penis, in the same way as the transvestists. They want the outside appearance of a woman in order to conceal, to deny, and thereby to protect the penis against any threat of castration.

If evidence is still required for the reality of the concept of castration anxiety, there is no more convincing observation point than the investigation of homosexuality and perversions in general. The same applies to the importance of the Oedipus complex in the psychogenesis of homosexuality, which can in many cases be deduced from the mere facts of the history.

My emphasis on the paramount significance of the Oedipal situation and of the related castration anxiety does not mean that I underrate the importance of pre-Oedipal conflicts and fears, arising from the early child-mother relationship as well as from a pre-Oedipal homosexual father-attachment. Nor do I wish to deny the presence and relevance of early wishes in boys to be like the mother and to possess female organs, although I should not place such desires in very early infancy. It is of great importance to realize that the homosexual's over-valuation of the male genitals, his 'penis-fetishism', so to

speak, can be a protective device to cover up deeper feminine desires. The cases of homosexuality where the pre-Oedipal mother-identification is strong, and where it is reinforced as the result of a negative Oedipus complex, are bound to be the most difficult to resolve. The same holds good for active homosexuals who continue to react in a strongly negative way to early disturbances of the child-mother relationship. These are often the patients who show marked schizoid or paranoid features.

The diagnostic and prognostic problem arising from these considerations is how to distinguish the homosexual whose condition is mainly determined by Oedipal conflicts and anxieties, from the homosexual whose problems date back further, namely to a severe pre-Oedipal disturbance. This is certainly no easy task, especially when we consider that nobody is likely to grow up without experiencing some difficulty at all stages of development. The relative significance of disturbances at the various levels may in certain cases be gauged from the history, and more accurately from the evaluation of other symptoms relating to particular levels. More reliable answers can only be obtained in the course of treatment itself.

Classification on the lines of the Kinsey rating scale (1948) according to the relative predominance of homosexual or heterosexual interest, can give useful prognostic indications, but is limited to conscious experience.

An exploratory analysis is thus the best means to meet the need for diagnosis in depth rather than by surface signs. It is obviously not advisable to start this trial with patients who are markedly psychotic or very resistant. With the majority of patients it is not difficult to explain that it is necessary to know more about their condition before one can make any definite recommendation, and that in psychological medicine—as indeed in some cases of medical and surgical treatment—exploration and therapy go hand in hand. This approach also helps to avoid complying with the

patients' demand for assurances about duration and outcome, which are even more out of place in the case of sexual perversion than in the analytic approach to other mental disorders.

Although psychoanalytic treatment is generally incompatible with the giving of active advice, I fully agree with the opinion held by Fenichel (1945) and others, that at a certain stage it often becomes necessary to remind the patient that his problems cannot be solved *in vacuo* whilst he continues to avoid facing the danger situation, in the same way as suggested by Freud (1919) for phobias. At times, particularly when the homosexual has improved sufficiently to seek and find a partner of the opposite sex, it may be very important to advise him to take his partner into his confidence rather than to continue avoiding heterosexual contact for fear of being found out through impotence. Much depends at that stage on the understanding of the partner. However, the need to guard against undue direction, especially in the early stages of treatment, cannot be overstressed. In all but the mildest forms of adolescent homosexuality a great deal of patience is required; and it is necessary to continue treatment over a sufficiently long period, even if the intervals between interviews are gradually extended.

The frequent presence of sado-masochistic features is an added complication, but not a contra-indication provided that they are not too severe. Unfortunately, the greatest difficulties are presented by the very type of homosexual whose treatment is the most urgent problem from the social point of view, namely the paedophile. In some cases psychotherapy can at least achieve a gradual rise in the age group of the desired partner. This can be seen as a spontaneous development when a homosexual who started by being exclusively attracted to smaller boys becomes more interested in young men as he grows up. In the course of treatment one can sometimes bring about a similar progress through the attainment of a higher degree of emotional maturity. In many cases the unconscious

identification with small boys is so strong, and the desire of such compelling force, comparable to an addiction, that the therapist is either placed into the position of an unwilling accomplice or into that of a parent who is consistently being deceived. In either case the therapeutic situation becomes impossible. Concurrent treatment by oestrogens to abolish or diminish sexual desire temporarily is not a satisfactory answer because it should not be given over long periods; moreover, this substitute for castration does not mix well with treatment directed at reducing castration anxiety. Cases of this severe type would seem to require prolonged institutional treatment; but here again it remains doubtful how far this can succeed *in vacuo*.

To sum up the conclusions from ten years' experience of psychoanalytically orientated psychotherapy of homosexuality, and of full

psychoanalysis in some cases: a fair number of patients can be helped to a certain extent; some can improve well beyond original expectation. Complete analysis remains the treatment of choice; but abbreviated or modified forms of analysis in well-selected cases can achieve satisfactory results. In general, the prognosis must remain guarded. The main justification for the psychoanalytic approach still rests in its unique research value. It cannot at present answer the great need for treatment on a larger scale. However, psychoanalysis has already supplied, and will continue to add to, the essential knowledge for more effective prevention of at least a proportion of the social ills and individual suffering in this field.

My thanks are due to the late Dr Denis Carroll for his advice and encouragement.

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PSYCHOSOMATIC PATHOLOGY*

By JOSEPH SANDLER

The construction of theories is very largely a matter of model-building. This is to say, we try to reduce the very complex processes which we study, to a relatively simple set of rules. Our simple model of what goes on is useful if it explains what happens, and if it enables one to make valid predictions and ultimately to exercise control over the observed phenomena

Such a model has been constructed for mental life by Freud, when he described the psychic apparatus and its functioning, and this model has been elaborated and modified, not always unanimously, by subsequent psychoanalytic theoreticians. A similar, and certainly far more adequate model exists for the functioning of the body, for its physiology, its biochemistry and its anatomy. These same models, with certain additions, serve to describe and explain pathological disturbances of normal functioning.

The existence of two more-or-less separate sets of theoretical models of this sort, one for the psyche and one for the soma would, I suppose, be quite satisfactory, were it not for the undoubted fact that psychological processes can create physical disorder and, conversely, that somatic disorder can produce psychological repercussions. But I wish to deal here with the problem of the psychogenic factor in somatic illness.

It is clear that we need to be able to bridge the gap between psychological conflict on the one hand and somatic illness on the other, and that we need to have a theoretical model capable of bridging that gap. It is not sufficient for us to say, for instance, that a patient's duodenal ulcer is psychogenic, without

our even being able to explain *how* the symptoms result from mental conflict. To call the organic lesion the 'materialization' of a phantasy is an insufficient theory. It is with the theoretical mechanisms which can bridge this gap between psychical conflict and somatic illness that this communication is concerned rather than with theories of specific causes.

Pathology has been defined (Willis, 1950) as 'the science which studies the causes of, and the structural and functional changes accompanying disease'. Willis divides the causes or antecedents of disease into two main classes, the first of which he calls 'abnormal environmental factors', and includes in this, physical trauma, parasites, harmful inanimate objects such as foreign bodies or toxins, nutritional abnormalities and carcinogenic agents. The second class he calls 'abnormal constitutional factors', and it includes not only genetic, but also non-genetic predisposing factors, such as previous defective nutrition or disease.

Notice that nowhere in this list is there provision for psychological disturbance as an aetiological factor in physical disease. We know that disturbances of bodily function *can* lead to disease, yet to the organic pathologist, if I may use this label, no provision is made for disturbance of function which results wholly or in part from purely non-organic psychological disturbance. And yet so many illnesses of unknown aetiology, not all of which have received the label 'psychosomatic', begin with a disturbance of normal functions which may be under psychological control. This is the point at which the organic pathologist finds himself at a loss. He is, as it were, on the somatic edge of the gap between psyche and soma. But whereas he can very often see nothing on the other side, the medical psychologist is in a favourable position to try to build bridges from both sides.

* Read to the British Psycho-Analytical Society, London, 1 May 1957. Based on a paper to the British Psychological Society, London, 24 November 1956.

In conversion hysteria, the hysterical symptom is thought of as having a symbolic significance; that is, it is a substitute expression of a forbidden wish, a wish which the patient's conscience does not allow and which remains unconscious. The hysteric expresses the unconscious desire in his bodily symptom, in what has been called 'somatic language'. The particular form in which it is expressed is determined by past experiences in which the forbidden wish played a part. What is often forgotten, however, is that the disturbed part or function *must have a representation in the mind*, whether this is in consciousness or not. This is, of course, a prerequisite for its having symbolic value. The functions which are best represented in this way are the normal sensory and motor functions such as seeing or walking, but certain involuntary and autonomic functions, such as vomiting or blushing, are represented in the mind as part of the subject's own idea of the structure and operation of his body. These involuntary functions, which at certain stages of development are more-or-less normal bodily activities, have been *perceived* and *cathected*, and are thus liable to the same laws of unconscious mental functioning as other perceptions. Hysterical blushing, vomiting, sneezing, diarrhoea, palpitations and shivering can all have the capacity to be symbolic representations of unconscious wishes. But those functions of the body which operate *silently* to restore the balance of our internal environment (that is, which restore homeostasis), cannot be involved in pathology as symbolic representations, unless they have been perceived and built into the mind's own model of its body. Hysterical identifications, for example with an ill parent, can provide the basis for hysterical symptoms, but this operates through the modification of the patient's own body schema and redistribution of cathexis to take in the new mental model of the affected parts.

Now this seems only psychological, and we can quite legitimately ask the question: What is the step between the psychological factors in symptom formation on the one

hand, and the somatic language on the other?

Hysterical disturbances of *sensation* need present us with no difficulty, as there is ample evidence to show that the distortion of sensory impulses takes place at cortical level—that is, distortion is by the ego, and we can consider the barrier in say, hysterical anaesthesia, as being comparable to that barrier which operates to keep repressed thoughts from becoming conscious. There is much evidence for this, and we need go no further than the examples—now rarely found outside text-books—of glove- and stocking anaesthesias, in which the area of lost sensation corresponds to a psychological rather than to an anatomical concept. More frequent are those cases in which a real physical trauma may cause a hypercathexis of the part or function affected, and which may form the basis for a later hysterical disturbance of sensation. But this is only part of the story, for it explains only hysterical distortions of perception. The gap between psyche and soma for hysterical *motor* disturbances (such as a fit) or *visceral* symptoms (such as vomiting or blushing) can be understood by a consideration of the relation between *phantasy* and *bodily activity*. To illustrate this relation, it is worth quoting the classical experiment of Jacobson (1932).

He was interested in what happened in the muscles of the body during the processes of thinking and imagination. By using very sensitive electrical measuring apparatus, he was able to demonstrate, quite unequivocally, that during the *imagination* of performing a particular motor act, there were measurable action potentials, and even microscopic movements, in the muscles involved in performing that act. If one actively imagined lifting one's right arm, the muscles which would be used in lifting that arm would be activated, although below the threshold of gross movement. By phantasying a motor act, we are, as it were, halfway to performing it.

It is no great step to generalize Jacobson's results to all ideation, whether conscious or not, and it follows that we must all be living

a more-or-less continuous motor- and visceral- life, in which, however, the adjustments are for the most part below the level of gross activity. When we phantasy riding a bicycle, biting an apple, telling a funny story, or blushing, the actual bodily changes which result in these activities will tend to occur, though this tendency may be below the level at which it produces a discernible effect. As the physical effects are so closely correlated with the psychic representation of these functions, we may assume that *primary processes* such as condensation and displacement can apply just as well to the bodily effect as to the ideational representation of that effect. In pathological disturbance of this normal mind-body relation, the result can be a hysterical symptom. To quote an example: A woman patient who had hysterical fits had spasmodic contractions of her leg which represented her unconscious phantasy-wish to have the penis and to be able to ejaculate like a man. The displacement from the idea of the penis to that of the leg followed the psychological laws of unconscious mental functioning described by Freud in the *Interpretation of Dreams* (1900). In this patient's phantasy life the leg was a displaced and symbolic representation of the penis she did not have. Her image of her body included a lost penis.

If we now turn to organic pathology, it becomes clear that most of the diseases which initiate in a disturbance of function and which we might nowadays call psychosomatic, are not hysterical. The change in function which produces symptoms is one which cannot have a representation in the patient's mind. While anyone can have an idea of, say, walking or vomiting, it is impossible for the mechanisms which regulate, for instance, the carbon-dioxide concentration in the blood, or the activity of the adrenal cortex, to have a mental representation in the body schema of what we might call the 'ordinary lay person'. These are what one can call the *silent* functions, most of which are under the control of the autonomic nervous system.

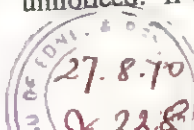
Consider the case of someone who pre-

sents with headache and vomiting, and upon examination is found to be suffering from malignant hypertension. In hypertension, there is evidence that, quite apart from a constitutional predisposition, the initial disturbance may be due to an prolonged and continuous vasoconstriction of the small arteries. This results in secondary changes in the circulatory system. The blood vessels lose their elasticity, and there are changes in the heart resulting from the increased work it is called upon to do. But vasoconstriction is a normal psycho-physiological process, relating, for example, to many of the processes of instinctual discharge. Now this patient must have been subject to a disease process for many years, a process of which he was not aware. Once the irreversible changes in his circulatory system have occurred, he will sooner or later develop symptoms—and these symptoms cannot be said to be a symbolic expression of mental conflict in the same way as might a hysterical headache or hysterical vomiting.

Such symptoms can, however, acquire either a new representation in the patient's mind or be hypercatheted, and so render themselves liable to be involved in subsequent neurotic processes. This is one of the reasons why it is always so difficult to disentangle the primary cause of this type of symptom.

There are a number of possible pathologies for such somatic symptoms without primary symbolic significance ('primary' is used here in a genetic sense). First we can consider the so-called *affect-equivalents*; that is, those bodily changes which occur as a result of dammed-up or undischarged effect. Many of these (such as increased force and frequency of the heart-beat) can be woven into a hysterical framework, for they are noticed by the patient, who modifies his body-schema and the distribution of cathexis in it accordingly.

Yet for every affect-equivalent which can be perceived, there must be thousands of functional changes in the body which go on unnoticed. If these proceed for long enough,



the whole body economy can be altered, and organic pathology can follow. Hambling, at University College, London, has been able to show quite convincingly, with patients who have a raised blood pressure, that the verbalization of repressed anger, with consequent discharge of that anger, produces striking falls of blood pressure (1951). In this sense, these patients have an *actual neurosis*, the effects of which proceed silently until disruptions of function or the following organic pathological changes, call themselves to the patient's attention.

While the affect-equivalents represent undischarged instinctual and affective tensions, there is a second group of silent pathological processes which are much more the specific expression of phantasies.

Selye has shown that profound internal adjustments take place when anyone is subjected to a situation of real external stress. The body's defensive reaction, typified in what he called the 'general adaptation syndrome' can cause various illnesses by attacking weak points in the patient's internal structure and economy.

Without gainsaying Selye, however, there is no doubt that there is a relation between the specific type of stressful situation in normal life and the body's response to that stress. Evidence for this is readily available from everyday experience, and from experiments with hypnotized subjects. The suggestion of extreme *cold* will produce such a degree of vasoconstriction that the subject will become pale and shivery, and indeed, if the suggestion is given strongly enough, his extremities will become blue. A suggestion of intense *heat* can produce vasodilatation and profuse perspiration, and even sunstroke. A suggestion of some specific *frightening* situation will give rise to still further involuntary responses. And it should not be forgotten that every involuntary adjustment observable to us represents only a fraction of the adaptive changes going on.

You may remember that the proposal was made earlier that we led a more-or-less con-

tinuous motor- and visceral-life concurrent with our phantasy-life. This would fall into the province of normal psychophysiology, but the existence of the normal motor- and visceral-life implies that when we *imagine* some threatening or frightening situation, there will be appropriate adjustments to prepare the body for meeting that situation, as if it really existed. These adjustments to a phantasied stress could give rise to the whole range of stress diseases described by Selye (1957), but it would also be related fairly specifically to the threatening phantasied situation. These would be *silent* processes, the body's reactions to a real or phantasied danger, and could give rise to *silent* organic pathology, which could ultimately produce symptoms.

Examples of this second type of reaction are cases of urticaria which Grace and Graham (Wolff, 1953) found to be specifically correlated with phantasies of being attacked, beaten or whipped. The body reacted to the phantasied threat as if it had really taken place. A further example is given by a male patient of my own who had strong and forbidden unconscious homosexual wishes, and who suffered from backache and painful feet, which were indeed flat. Analysis revealed that he was continuously keeping his buttocks pressed together in order to defend himself against his own wish for anal penetration, and that this defensive activity led to a postural imbalance which gave him a very real backache and put strain on his feet. His symptoms had had no primary symbolic significance, though they were, of course, embroidered into his phantasies.

If we return to the list of physical causes of diseases quoted earlier, it is clear that, quite apart from hysterical phenomena, disorder of the body's function due to the prolonged effect of undischarged affect would have to be included. Further, while foreign bodies, parasites, toxins, and nutritional abnormalities may cause disease, the phantasies of such threats to the body's integrity might do so as well, though we certainly cannot postulate

a one-to-one correspondence between the effects of reality and those of phantasy.

The possibility of certain benign or malignant tumours being reactive to the body's physiological imbalance (due in turn to psychological causes) cannot be easily dismissed. There is, in fact, strong evidence that psychogenic factors can play a part in the formation and growth of tumours (Leshan & Worthington, 1956). It is the experience of some surgeons that women with cancer phobia and, in particular, with fears of cancer of the breast, have in a significant number of cases microscopic cancers, which in many cases could have originated after the onset of the phobia. Widows tend to develop cancer of the colon with greater frequency than a comparable group of married women, and this appears to be related in time to the loss of the husband. One does not yet know the mechanism in these cases, but it could conceivably be the result of undischarged affect, or reactive to specific groups of phantasies.

To conclude: It has been the aim of this short paper to describe possible mechanisms for bridging the gap between psychic conflict and somatic symptoms. It has not been my aim to answer the question of why some people develop psychosomatic illness and others do

not. I would like to say, however, that it should not be forgotten that the emergence of a symptom can create a *false* psychopathology, quite different from that which started the initial functional disturbances which finally led to the symptom. It would be quite wrong to assume that the manifest psychopathology is always the true one. Once the symptom is conscious, there follows a reorganization of the whole personality, *exactly as if the patient were ill from physical causes*. And indeed, the final outcome, from the point of view of mental economics, may be quite different from the state of affairs which obtained at the time of the commencement of the pathological process, a process which after a period of time produced manifest symptoms.

I have not presented this thesis in terms of object-relationships, because I have not been concerned as much with the content of phantasy as with the mechanisms whereby ideation can produce somatic dysfunction. I would also like to say, that in describing these mechanisms, I do not for one moment believe that any disease has a single cause, but for the purposes of the theoretical fragments presented here, only the mode of functioning of psychological factors has been taken into account.

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PATTERNS OF ANXIETY: THE CORRELATES OF SOCIAL ANXIETIES*

BY JOSEPH SANDLER, CECILY DE MONCHAUX AND JAMES J. DIXON

In a previous study of the patterns of social anxiety (Dixon, de Monchaux & Sandler, 1957), it was demonstrated that in a group of twenty-six items of the Tavistock Self-Assessment Inventory, all of which referred to one form or another of social anxiety, there existed a strong general factor and four group factors. The general factor was identified as *general social anxiety*, and the four group factors were labelled as follows:

- A. *Social timidity.*
- B. *Fear of loss of control, especially bodily control.*
- C. *Fear of exhibitionism.*
- D. *Fear of revealing inferiority.*

The original statistical analysis was based on 250 adult cases at the Tavistock Clinic, and following the analysis of social anxiety into its components, the four group factors were each related to the remaining 850 items of the Inventory, in order to amplify the classification reached by the statistical analysis. The correlates of the factors were calculated using a population of 200 cases, by a method which has been described elsewhere (Dixon, 1955). The correlates were found for men and women separately, and a large number of items of the Inventory were found to be statistically significantly associated with the

four group factors, at the $p=0.01$ level or beyond. This paper will be concerned with the description of the correlates of the factors, and with an assessment of their significance. In order to sharpen the clinical picture associated with each of the group factors, the practice was followed of utilizing only those items significantly associated with the factor in question, but not significantly associated with any of the other three factors.

A. SOCIAL TIMIDITY

The items most highly saturated with this factor in the original analysis were, in order of their factor saturations:

- 26. I usually feel awkward with strangers.
- 11. I usually feel uncomfortable when with a crowd of people I do not know.
- 10. I usually feel nervous when speaking to someone in authority.
- 19. I find it difficult to ask other people for information.
- 18. I always feel uncomfortable when I do not know what is expected of me.
- 15. I am usually in doubt whether to greet someone I know only slightly.
- 24. I am sometimes afraid of expressing myself in case I make a foolish mistake.
- 25. I feel generally uncomfortable when eating or drinking in front of others.

* This paper is the eighth of the Studies in Psychopathology using a Self-Assessment Inventory. It is the third of a series of papers which includes work submitted by Dr Dixon for the degree of Ph.D. in the University of London (Dixon, 1955). The present research was carried out jointly at the Tavistock Clinic under the direction of Dr Sandler and at the Psychology Department, University College, London, under the supervision of Dr de Monchaux. Manuscript received 1 June 1957.

These items were all thought to refer to 'social situations in which the subject is self-consciously afraid that he will not know how to behave, especially in strange social surroundings. They refer to a type of social inhibition in which the accent is not upon intrinsic inferiority, but rather on the fear of creating, in strangers or in authority figures, an adverse impression'.

The results of an examination of the correlates of this factor are given for men and women separately. The detailed lists of correlates have been given by Dixon (1955). For the purposes of description, the material will be referred to the 'typical' man or woman, the 'typical' pictures being drawn from the study of the list of those items of the Inventory found to be correlated with each factor.

The male of type A

The items significantly associated with factor A confirm the original impression of this tendency as being one of social timidity. The typical man of this group is solitary, rarely or never enjoys going to parties and describes himself as being the sort of person who thinks much and speaks little.

It seems clear that the tendency to withdraw from social contact follows from his difficulty in assuming an active, dominant social role. Thus he feels that he has little influence on others, does not like organizing things, tends to give in rather than continue a quarrel and hates saying anything which might lead to a scene. He seems to be extremely conscious of the possibility of committing a social error, for he tends to go over his past behaviour thinking of different ways in which he could have acted, and tends to brood for a long time over a single idea. It would seem that he becomes flustered in company, for he forgets the names of people he meets and acts on the spur of the moment without stopping to think. His memory 'lets him down', and he fails to pay much attention to detail. Not surprisingly, he finds it difficult to feel affection for others and has few friends. He feels 'hopeless' at sports and does not take part in athletic games. Strikingly, however, he enjoys playing with children.

Although the picture presented here is that of a man who is timid in the presence of others, it appears that his timidity is not simply a negative quality, due to the absence of some active component in his personality, but is the result of a widespread inhibition in the assumption of a socially active role.

The female of type A

In general, it can be said that the woman who has a high score on the factor of social timidity is similar to her male counterpart. She is socially ill-at-ease, uncomfortable if she has to greet someone in the street, finds it difficult to speak in public, and is bothered if watched at work. She does not enjoy meeting people, rarely enjoys parties, forgets the names of people she meets, and dislikes it if a stranger tries to strike up a conversation.

She finds it difficult to assume a dominant role, being afraid of responsibility. She dislikes and feels that she is no good at, organizing things, prefers others to take the lead and feels that she lacks will-power. She has not the strength to stand up for her rights, and tends to give in rather than continue a quarrel. If she has to explain something she gets very flustered, easily loses her train of thought and fails to cope with simple tasks. She is easily persuaded to do things she does not want to do, and feels that she is very gullible and easily taken in. She feels embarrassed if she has to tip someone, is easily tongue-tied, and is embarrassed when someone does her a favour. She is the sort of person who thinks much and speaks little, tends to talk less than the average when in company, and dislikes new and unaccustomed things. She tends to chew up the ends of pencils or knitting needles.

A passive, pessimistic attitude towards success is revealed by items which refer to a hopelessness about making a success in life. She believes that success is more dependent on good luck than on hard work. She feels inferior to others, feels that some part of her body is too big or too small or of the wrong shape, and is embarrassed when seen naked by members of the same sex. The sight of blood upsets her, and she is uncomfortable in the company of a cripple or anyone with a physical defect.

For both sexes, the socially timid person is revealed as an inhibited person, the main area of inhibition relating to the assumption of a dominant social role. The main differences between the women and the men are reflected

in the woman's expression of a passive-pessimistic attitude to life.

B. FEAR OF LOSS OF CONTROL, ESPECIALLY BODILY CONTROL

The items which had the highest saturations with this factor are as follows:

9. I am often worried in case I might vomit or be sick in public.
14. I sometimes have the fear of fainting in public.
13. I sometimes worry that I may want to pass water at an inconvenient time.
16. I find it intolerable to be on bad terms with someone in authority.

These patients were felt to be 'anxious lest they fail to retain control over certain bodily functions.'

The male of type B.

The main feature presented by the correlates of this factor for the men is a small group of fairly non-specific items relating to the body. The typical male fears that there is something the matter with him which doctors have failed to discover, has pains which move from one part of his body to another, has to pass water unduly frequently, and feels that work is a terrible strain. He dislikes being touched by women and having his hair cut.

The female of type B

The women show a larger group of symptoms relating to the body and its organs. The typical woman finds herself worrying about the possibility of getting or having some terrible disease, sometimes feels as if she might faint, suffers a lot from wind or gas, has attacks of dizziness, perspires even when she is not hot, and fears that she might choke. She cannot stand long periods of hard work. She is ashamed of her sexual organs and feels that every task she faces is somehow a test for her.

She also shows certain obsessional features. She feels the compulsion to count things, takes great care in hanging or folding her clothes at night, pays a great deal of attention to detail,

and has a feeling of contempt for people who are slovenly in their dress or in their behaviour. Unlike the socially timid woman, she gets on well with others, though she continues a quarrel rather than give in. She does not, however, provoke opposition and gets a great deal of conscious pleasure out of being polite to people. She sometimes feels, without knowing why, that something terrible is going to happen, but she believes in the power of prayer to help her overcome her difficulties.

The women, in addition to their concern about the state of their bodies, present a mild obsessional picture, but not one severe enough to justify a diagnosis of obsessional neurosis.

C. FEAR OF EXHIBITIONISM

The items which defined this factor originally are:

12. I feel uncomfortable when I am the centre of attention (e.g. when crossing a dance floor).
7. If I come late to a meeting I would rather stand than take a front seat.
5. I hate walking past a crowd of people.
23. I feel very embarrassed if there is anything wrong with my clothes or appearance.
22. I feel nervous about performing in front of people.
20. Talking to people about my personal appearance makes me acutely uncomfortable.

This factor was felt to refer to a 'fear of being noticed by others...a fear of self-display, for in most of the items in this group there is an implication of the patient's doing something which might draw the attention of others to him.'

The male of type C

The initial definition of this factor as one of fear of exhibitionism is substantiated by such correlates as a feeling of embarrassment when seen entering or leaving a lavatory, or when in a crowded place. The typical man in this group feels that people in the street look at him

critically. He feels more comfortable with people he knows than with strangers and he is very sensitive to interference by others in his affairs. He admits to hostile feelings and gets a sneaking feeling of pleasure when people he dislikes get into trouble. He enjoys being 'in charge' of situations involving other people, and feels that others do not realize what an important person he really is. He prefers to ignore rather than attack an opponent, disapproves of sports involving the killing of animals, and is upset at the sight of blood. He is not hopeful about making a success of life and has no philosophy of life which helps him much. Relatively minor matters worry him, he has a slight feeling of contempt for people who are slovenly in their dress or behaviour, and is fussy about keeping his hands clean.

He is embarrassed when told a dirty story, dislikes the smell of perspiration, is much upset by unpleasant smells, and particularly enjoys the scent of perfume or flowers. His eyes tire easily, but he is specially fond of window shopping. He tends to get short of breath without exertion and sometimes pretends to be ill in order to get out of doing something.

He dislikes the masculine sort of woman, and thinks that he has a strong streak of homosexuality.

The female of type C

The typical woman of this group is embarrassed when she has to be examined by a doctor, keeps her private feelings hidden behind a wall of reserve, and is uncomfortable when seen naked by someone of the opposite sex.

She frequently gets pains near the heart, is often troubled by her head throbbing, and sometimes feels like vomiting when excited or nervous. A tendency to hypersensitivity and phobic reactions is evident in a dislike of bright colours, a fear of knives, insects and of going to the dentist, a readiness to be irritated by people, a feeling of annoyance when others complain of their hard lot, and impatience.

She dislikes the masculine sort of woman and believes that it is no use trying to go against one's destiny or fate. She generally feels obliged to give money when she passes a beggar in the street, but does not get deeply and emotionally involved with others. Her memory often lets her down.

D. FEAR OF REVEALING INFERIORITY

The items delineating this factor are as follows:

1. I often have the fear that others might think me unintelligent or ignorant.
2. I sometimes get the fear that my clothes are not properly done up.
3. I am afraid of being disliked by people.
6. I constantly seem to feel that I have offended someone.

These items were taken to refer to 'fears of being *judged critically* by others... in contrast to the items listed under A, items which refer rather to subjective feelings of discomfort, awkwardness or tension in the presence of others.'

This factor had the highest number of correlated items for both sexes.

The male of type D

The typical male of this group presents a rather unhappy picture of embittered irritability. He feels that he is sometimes blamed or punished unjustly, that he is often cheated, and always seems to be having bad luck. He sometimes has the feeling that he is being followed by people who wish to harm him, and occasionally has the thought of being attacked from behind. He tends to doubt what others tell him till he can see for himself, and believes that the more he wants something, the less chance he has of getting it. He feels that he has never been given a real chance in life, and that people take an unnecessarily long time to recognize the value of his work. He sometimes feels that others can read his thoughts.

In addition to these persecutory ideas, he also shows many aggressive features. He sometimes feels that he wants to hurt or injure

animals or people, and sometimes feels like destroying or smashing things, and has the fear that he might in some way hurt a child. He thinks that it is always right to strike back if attacked, and feels that wrongs must be repaid.

A number of items reveal a masochistic tendency. He often injures himself accidentally, thinks that masturbation may harm his health, and worries about being accidentally killed. These are associated with a large group of depressive items. His mind dwells a good deal on death, he finds it difficult to lead a happy life because of mental conflict, and sometimes has a guilty feeling without exactly knowing why. He feels that some part of him is wicked, and that he does not suffer enough. He is troubled by bad and dirty thoughts, feels ashamed of his dreams, and believes that it is no use going against one's destiny or fate. He finds it difficult to have any sort of pleasurable feeling, and worries in case someone he is fond of will die. He reports a number of fears referring to death, drowning, and the thought of being left alone, particularly in a large open space. He also feels that his brain does not function as well as it used to, that he tends occasionally to do things in a dream-like state. He is often away from work through sickness, and he feels that he is temperamentally different from other people.

His irritability is shown in his impatience, his excitability, the fact that he is easily put off if things do not turn out as he wishes, and is annoyed if offered advice.

He thinks it is bad to have statues of nudes in public places, but easily finds himself showing off to others. He has day-dreams of doing something really big, but feels that he often gives other people quite a wrong impression of himself. He strongly resents playing 'second fiddle' to anyone, and admires dominant personalities. He often feels superior to other people, and tends to make biting or sarcastic remarks when criticizing others. He does not get on well with others at work, and sometimes feels resentful of those in authority. He feels that it is no use doing

things for people—one only wastes one's time.

When away from home he is usually concerned about when and where he gets his meals, and has a great love of physical comfort. He feels that he does not get the right sort of food, but that he is greedy about food. He feels that his sexual urges may be too strong, and he tends to hoard things for the future.

The female of type D

The women of this group have a greater number of complaints than the men, and the general picture is that of a person who is even more disturbed than her masculine counterpart. She often feels that the whole world is against her, that people take advantage of her friendship and say rude and insulting things about her. She sometimes feels that others hate her, and that they can read her thoughts. She has a strong dislike for inquisitive people, and feels that people take an unnecessarily long time to recognize the value of her work. She sometimes feels resentful when others are more successful.

In contrast to these persecutory ideas, outwardly directed aggressive feelings are not very marked, except in the few items 'I sometimes hate someone very much', 'I get a sneaking feeling of pleasure when someone I dislike gets into trouble', and 'I frequently find myself disagreeing with and contradicting other people'. The overtly masochistic features characteristic of the men are absent, though these are more than compensated for by feelings of unhappiness, guilt and depression.

She feels that she is a bad and unworthy person, that she never gets what she really wants. She sometimes contemplates taking her own life and has to put thoughts of death out of her mind. She feels that life is full of misery, pain and hardship, and she cannot get any satisfaction out of the things she does. She has no real friends, suffers acute mental conflict, feels that life is just a wild goose chase and she sometimes wishes she were dead. She feels that others have little or no idea how much she suffers, and she sometimes gets

depressed because she feels that she has done wrong. She gets feelings of guilt without really knowing why, experiences strong pangs of conscience, and fears that she does not live up to the standards of her conscience; yet she feels that it is not as strong as it ought to be. She feels ashamed of her dreams and ashamed of herself, and broods for a long time over humiliating experiences. She feels that people would despise her if they really knew her.

A group of items refers to disturbances of perception and cognition. She is occasionally unable to see or hear for a while, even though awake. She sometimes has a feeling of separation from her own body, and often feels as if things were just not real. She sometimes seems to lose all sensation in her body, and does things in a dream-like state without remembering afterwards what she has done. There are times when she loses control over her actions, although she is quite aware of everything happening at the time. She sometimes has 'queer' feelings in some part of her body.

She believes that she can stand more pain than other people, but is sometimes afraid that she might go mad. She thinks more about her private feelings than of the practical demands of life, but feels that most of her interests are scientific and technical. She finds that she has to stop and think before doing even the smallest thing, and sometimes has to memorize numbers or count things that are not important. She feels that her brain does not function as well as it used to and her mind wanders so badly that she loses track of what she is doing.

She tends to delay paying bills even though she has the money to pay them with, has a strong wish to take things which do not belong to her, becomes stubborn or resistant when others tend to force her, and strongly resents playing 'second fiddle'. Discipline makes her discontented.

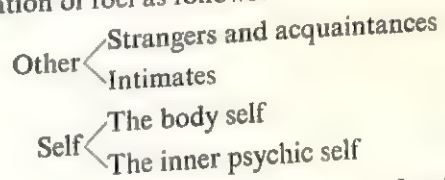
Her irritability is shown by frequent excitement, a feeling of restlessness and an inability to concentrate for long on anything. She talks in her sleep and her hand shakes.

An area of exhibitionism is revealed in her tendency to exaggerate things in order to impress others. She feels dissatisfied if she remains unnoticed, and wishes that she were the sort of person whose picture often appeared in the newspapers. She is upset when people she knows fail to recognize her in the street. She is concerned with herself and her position in the world most of the time, feels that others have not counted much in her life, and spends most of her spare time on her own. She is apt to complain of her hardships.

Somatic complaints include constipation, backache or heaviness of the limbs, and she sometimes gets a feeling of pressure on top of her head or the back of the neck. She gets sudden cravings for food at odd times.

THE FOCUS OF CONSCIOUS ANXIETY

A consideration of the four group factors and their correlates suggests that the abundance of material we have obtained might best be ordered by considering the *focus of conscious anxiety* in each of the four cases. The material appears to fall naturally into a fourfold classification of foci as follows:



These represent a continuum of critical stimulus-situations; situations which are subjectively located either within the psychic self or at increasing distances from it, i.e. within the body self, in social situations involving intimates, or in those involving strangers and acquaintances.

Table 1 shows the relationship obtaining between the four group factors and their correlates on the one hand, and the focus of conscious anxiety on the other. It also includes a column in which some aspects of the psychiatric significance have been summarized.

From table 1 it may be seen that the extent of the associated disturbance is related to the focus of conscious anxiety. In type *D*, where

Table 1. *Focus of conscious anxiety*

OTHERS ←

→ SELF

Type	Factor	Others (Acquaintances and strangers)	Intimates	Body-self	Psychic self	Psychiatric significance
A	Social timidity	Main focus of anxiety. Inhibited in taking social lead. Timid and afraid of giving a <i>wrong</i> impression. Awkward with strangers. The women have in addition a passive pessimistic attitude towards success (a pessimism about themselves as whole persons rather than an inner feeling of 'badness')	Not gregarious, but inhibitions relate mainly to strangers rather than to intimates	No special problem. The women are embarrassed when seen naked by members of same sex, but this is, however, more a socially located anxiety	No feeling of 'badness' about self. They get flustered and do not function well mentally in company (i.e. they react to <i>social</i> stimulation in this way). No real defect is felt in the self	No positive significance, apart from the tendency to be inhibited
B	Fear of loss of control, especially bodily control	While these people are relatively at ease in the presence of others whether strangers or intimates, they fear involuntary bodily expressions in public		Main focus of anxiety. Anxieties relate mostly to body functions. They are hypochondriacal and easily fatigued	Absence of manifest conflict focused here	Mildly obsessional anxieties present in the women. Of general psychosomatic significance
C	Fear of exhibitionism	Main focus of anxiety. Fears relate to self-display and the danger of being seen by others, particularly by a group. Marked 'stage fright'. In the men, 'pseudo-paranoid' features really relate to exhibitionistic conflicts.	No apparent problems	The men are rather sensitive (e.g. to smells), and have a few 'effort' symptoms. The women have fears relating to exposing the body to males. Some hysterical conversion symptoms.	No special focusing of conflict on this area	No specific psychiatric syndrome for men. For women, a slight tendency to hysterical conversion and phobic anxiety
D	Fear of revealing inferiority	Ideas of reference and persecution. Aggressive feelings towards others, men showing this more than women (the persecutory ideas are not dependent on the presence of others).		No special symptoms for men. Women have a number of aches and pains	The main focus of anxiety. Acute guilt and depression with inferiority feelings reflecting strong self-hatred. In women, a considerable degree of depersonalisation.	A widespread disturbance exists, not confined to social relationships, and which has marked depressive and persecutory features. In women, there exist symptoms of depersonalization and derealisation. It seems probable that patients of this category are more likely to be involved in subsequent psychotic illness, and will respond less well to psychological treatment.

the emphasis is on morbid internal self-pre-occupation, there is a widespread correlated psychiatric disturbance. It is important to note that the members of type *D*, who report ideas of reference in addition to their depressive and guilt-saturated symptomatology, have their focus of conscious anxiety predominantly within the psychic self. Their persecutory ideas (e.g. the feeling that the whole world is against them, that others hate them and can read their thoughts and 'that they are being followed by people who wish to harm them') are not absolutely conditional on the presence of others. We may compare this type of paranoid persecutory idea with the 'pseudo-paranoia' of group *C*, where the essential anxiety relates to being seen by others.*

Within the region of the self, the distinction between the body-self as the focus of conscious anxiety and the psychic self is brought out by the comparison of types *B* and *D*. The widespread psychological disturbance of group *D* contrasts with the narrower range of predominantly psychosomatic symptoms. Finally, comparing the extremes of our continuum, type *D* may be contrasted with type *A* in which there appear to be few correlated symptoms, no feeling of inner 'badness', and the anxiety is absolutely conditional upon the presence of others—especially strangers.

* This is a distinction which is unfortunately not always made in clinical practice. Many patients, essentially of type *C*, receive the label 'paranoid', and yet have a pseudo-paranoia of the 'stage-fright' variety.

In this discussion we have not attempted to present our interpretation of the very rich personality pictures in terms of the psycho-analytic view of personality development. The testing of the exact relation between the patterns distinguished here and early infantile precursors must await research involving longitudinal studies of children. Our hypothesis is, however, that the classification of crucial anxiety-provoking situations reported here is a reflexion, in adult life, of certain well-established social situations of childhood. To take but one example: the strong association of the fear of loss of bodily control (factor *B*) with the fear of being on bad terms with someone in authority* can be understood in terms of a fixation at the infantile social situation of the child who is afraid of incurring his mother's displeasure by soiling. The wish to soil (i.e. to relax bodily control) and the fear of loss of control, based upon the fear of the disapproval of a parental authority figure, finds expression in a generalized version of an infantile social anxiety.

In conclusion, we wish to stress the potential value of the concept of the *focus of conscious anxiety* for diagnosis, prognosis and research, for an abundance of clinically relevant information may lie concealed beneath a relatively small superficial difference in the expression of a social anxiety.

* Of the four items which originally defined factor *B*, the first three relate to loss of bodily control in public, and the fourth is the 'authority' item.

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THE ASSESSMENT OF PROGRESS AFTER AT LEAST TWO YEARS OF GROUP PSYCHOTHERAPY

By HERBERT PHILLIPSON*

The research reported in this paper was undertaken as a preliminary investigation to provide an estimate of progress achieved by group psychotherapy, and as a means of formulating hypotheses which could be the subject of later experiments, concerning the composition of groups in relation to therapeutic success, and the types of patients who do or do not benefit from group treatment. It was also hoped that the experience gained in the study would lead to a more satisfactory procedure for the assessment of progress in psychotherapy.

In this report the main emphasis will be on the first of these aims. Only general conclusions from other parts of the study will be presented.

The study was undertaken in a large outpatient psychiatric clinic which specializes in psycho-analytically oriented psychotherapy. Patients tend to be drawn from professional and semi-professional strata and the population is therefore highly selected. Those patients who are given group psychotherapy fall within the range of 'good average' to 'very superior' in intellectual ability, approximately 85% being in the 'superior' and 'very superior' categories. The age range is from 19 to 54 years, with 31% between 19 and 29 years, 45% between 30 and 39 years, and 16% between 40 and 49 years. Groups are formed from a waiting list to which suitable patients are assigned after an initial diagnostic interview. This is done on the basis of roughly equating intelligence level and educational background. Apart from these considerations, which are applied to facilitate communica-

tion between members, no other criterion is uniformly used, although preferences by individual therapists do operate in the selection of patients for their groups.

The technique of therapy employed derives from the psychoanalytic understanding of the personal relationships which develop within the group, and between the group, its individual members and the therapist. Although there are some variations in the details of the technique used by the therapists, all agree on the fundamentals of their approach, emphasizing the importance of transference interpretations as the mainspring of the therapeutic process. The general approach follows that described by Ezriel (1950, 1952) and by Sutherland & Ezriel (1952).

The duration of treatment and the samples studied

All but one of the groups included in the study were conducted on a once-weekly basis. The general view of the psychiatrists taking these groups is that the more radical changes in the effectiveness of personality functioning do not usually show until after two years of group treatment, although in many cases relief of symptoms may be noticed at earlier stages. For this reason, and also to ensure that evidence on the nature of changes resulting from therapy would be more firmly based, the study was restricted to those groups which had been in treatment for a minimum of two years.

The study was commenced in the autumn of 1955, when there were about 65 groups in treatment at the Clinic. Of these, 21 had been in treatment for two years or more. Groups are usually started with eight or nine members, and new members are added to fill vacancies

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Table 1

	Group code	No. of patients assessed	Total no. attending group during the period	Length of period of treatment			
				2 years or more	1-2 years	6 months-1 year	Under 6 months
Dr A	(a)	8	11	8	1	0	2
	(b)	5	9	5	2	0	2
	(c)	6	10	6	1	0	3
	(d)	6	16	7	2	0	7
Dr B	(a)	6	11	7	2	0	2
Dr C	(a)	6	17	9	0	2	6
	(b)	6	14	6	1	2	5
Dr D	(a)	4	14	4	4	1	5
	(b)	4	9	4	2	2	1
	(c)	8	11	8	1	1	1
Dr E	(a)	4	9	7	2	0	0
Dr F	(a)	—	11	5	4	1	1
	(b)	—	14	7	1	1	5
Dr G	(a)	—	8	7	0	0	1
	(b)	—	10	5	3	0	2
Dr H	(a)	—	8	4	2	1	1
	(b)	—	7	4	3	0	0
	(c)	—	16	8	1	3	4
Dr I	(a)	—	10	6	3	0	1
	(b)	—	9	6	2	1	0
	(c)	—	14	6	3	2	3
Totals A-E	11	63	131	71	18	8	34
Totals F-I	10	—	107	58	22	9	18
Totals A-I	21	—	238	129	40	17	52

at varying intervals. Table 1 shows the total composition of these groups and the numbers remaining in treatment for varying periods.

The 21 groups totalled 178 patients at the beginning of treatment, and as a result of admitting new members from time to time 238 patients were treated in these groups. It will be seen that between one-third and one-quarter left the groups before completing six months of treatment. Detailed information on the reasons for a patient leaving his group is not available in many cases. However, from existing records these patients appear to fall into two broad but not exclusive categories: those who were too acutely anxious to tolerate

the group situation, and those suffering from some transient condition which remitted very early in treatment.

It is of interest that less than one in ten left the group after being in it for periods between six months and one year. About one-sixth left the group after being in it for more than one year but for less than two years.

Of the 238 patients, 129 completed two or more years of treatment. Sixty-three of those, who were still in treatment in October 1955, were the subjects of this study. They represent the patients of five therapists (A-E), who were able to complete detailed progress reports within the ensuing eight months. Notes on the

composition and functioning of 10 of these 11 groups were also obtained, and on five other groups among those taken by doctors F. I.

In addition, from this group population of 129, a sample of 30 cases, drawn at random, was reported on by the doctor in charge of each group, and also independently by another doctor (X) after an interview of at least 1½ hours. A third doctor (Y) who also had no previous knowledge of the patients, made an independent retrospective assessment of the degree of severity of illness at the beginning of treatment, from his examination of the record of diagnostic interviews. (Drs X and Y actually reported on 29 cases, since one patient failed to appear and no suitable substitute was available.)

The methods used for assessing changes

Since a two-year period of treatment was taken as a minimum for the study, it was clearly impossible to obtain a parallel untreated population, matched also for symptomatology and degree of illness, for comparative purposes. In particular the severity of the disturbance in many of the group patients would make such a procedure impossible. Equally, it was impossible for ethical reasons to seek information about progress from extra clinic sources.

The method used was restricted to a careful recording of evidence from the group treatment and from independent interviews. It should be emphasized that the data available to a therapist after observing a patient in a group for two years or more are very different from those which could be obtained by asking the patient for his own assessment of progress, or from another's clinical impression. Throughout the work of the group patients tend to comment frankly on the contributions and behaviour of other members. Thus it may be that a member is perceived by the therapist as one whose contributions are destructively critical of the rest of the group or parts of it. This may be felt to be so by the group and may be verbalized in these terms. Equally a change in the quality of such a member's contributions to a more

constructively critical approach may be shown by the direct comment of fellow members. It is also usual for the group to develop some social life of its own outside the clinic, even to the extent of visiting between families. Changes in social behaviour, in interest-involvement, or even in family relations may be commented upon by other members of the group, based on their own observation. The conglomerate of this type of evidence, in addition to the therapist's own observations, yields a wider range of information than would be obtainable from direct questioning of the patient.

The doctors concerned in the research were required to record evidence of changes in five areas of the patient's life:

- (a) Symptoms and complaints.
- (b) Quality of relations in the group.
- (c) Effectiveness in work life.
- (d) Quality of heterosexual relations.
- (e) Quality of general social relations.

On the basis of the recorded evidence they were then required to give a rating of progress in each of the five areas on a 7-point scale:

1. Considerably improved.
2. Definitely improved.
3. Some slight improvement.
4. No apparent improvement but appears to be getting something from the group.
5. No improvement.
6. Worse
7. Breakdown.

From the total evidence an assessment of overall improvement was also recorded on this 7-point scale.

In addition ratings of degree of disturbance in the five areas were made, for symptoms, on a 3-point scale.

1. Very severe.
2. Moderately severe.
3. Mild.

For the remaining four areas, on a 4-point scale:

1. Very severe (incapacitating or nearly so).
2. Severe (marked difficulties).

3. Mild (difficulties which do not seriously impair relations).

4. No real problem in this area.

In order to reach optimum agreement on the criteria, and to obtain a common basis of assessment, each doctor worked through two of his cases in detail with the psychologist. The evidence was discussed and recorded, and the ratings made, after at least 1½ hours' consideration of each case. Thereafter the doctors completed other cases on their own. Similar discussions preceded the independent interviewing of the random sample of 29 cases.

from coming into contact with him', would not be rated as improved at 1, 2 or 3 level. If this behaviour had been sustained, without response to interpretation he might be rated as 5 (no improvement) or 6 (worse).

In order to illustrate the evidence summarized by the doctors, a completed report form is reproduced as an Appendix.

RESULTS

Table 2 gives the number of patients (rated by the doctors in charge of groups) showing the various degrees of change in the five areas

Table 2. $n=63$

7-point scale	Symptoms	Group relations	Work relations	Heterosexual relations	Social relations	Over-all
1	3 (1)	2	2 (2)	1 (1)	1 (1)	2
2	27 (8)	22 (20)	20 (13)	13 (10)	20 (13)	25
3	21 (9)	21 (14)	18 (9)	21 (18)	17 (11)	22
4	9 (4)	13 (10)	14 (7)	23 (19)	18 (12)	11
5	1	4 (3)	8 (2)	5 (4)	6 (3)	2
6	2 (1)	1	1 (1)	—	—	1
7	—	—	—	—	—	—
Totals 1+2	30 (9)	24 (20)	22 (15)	14 (11)	21 (14)	27
Totals 1+2+3	51 (18)	45 (34)	40 (24)	35 (29)	38 (25)	49
Totals	63 (23)	63 (47)	63 (34)	63 (52)	62* (40)	63

* A rating of change in this area was not made for one patient because of grossly conflicting evidence.

Ratings of 'considerably improved' or 'definitely improved' were given only where the evidence seemed clear, and where the improvement was marked in at least one area involving relations with people. For example, a patient who showed marked diminution of a severe phobic anxiety and had sustained this improvement over at least six months, would not be rated 1 or 2 in over-all improvement unless there was also evidence of improvement of the effectiveness of his social relations. Without this, the rating in such a case would be 3. Again a patient about whom the doctor recorded under (b) (quality of relations in the group): 'He says a lot more than he used to do with a certain amount of vehemence, but in a manner which excludes the rest of the group

considered, as well as in their over-all progress. The number in parentheses refers to those cases (as rated by the same doctors) who were assessed as having very severe symptoms (rating of 1 on the three-point scale for symptoms) of having severe or very severe difficulties in the other four areas (ratings of 1 and 2 on the four-point scales), at the beginning of treatment.

The ratings took eight months to complete, and during this time no patient broke down and had to be admitted to a mental hospital. From the eleven groups studied, two patients had previously broken down during treatment. A total of six patients in the 21 groups (238 patients) considered in Table 1 broke down during treatment.

Forty-three per cent (27 cases) of patients are rated as showing an over-all improvement which is marked and clearly evidenced (ratings of 1 and 2). A further 35% (22 cases) have improved to a less marked extent (rating of 3). Of the five subareas, most improvement is shown in the area of symptoms, almost half the cases being assessed as considerably or definitely improved (ratings of 1 and 2). If slight improvement in symptoms is included (rating of 3), the improved cases total 81% (51 cases). The lowest figure for improvement of any sort is 55% (35 cases), for the area of heterosexual relations. It is not clear whether changes in this area are relatively difficult to make, or whether the relevant information was less readily available in the group sessions.

half of the sample of 29 cases had an additional period of treatment of six to eight months before the independent interviews were carried out. Secondly, the results may be influenced by the fact that of the 30 cases randomly selected for the independent interview, only 23 attended at first. In five of the absent cases a first reserve, and in two cases a second reserve choice was substituted. Although these reserves had been selected when the original 30 were chosen, in order to meet this possible contingency, some of these reserves may represent more co-operative and possibly less ill patients than those in the original 30 who did not make themselves available for interview, as the fact of non-attendance and severity of illness are probably correlated. Finally 29 cases were assessed.

Table 3. $n=29$

7-point scale	Dr X		Dr Y Poor prognosis
	Symptoms	Over-all change	
1	3	3	0
2	12	13	7
3	11	9	3
4	3	4	3
Totals 1+2	15	16	7
Totals 1+2+3	26	25	10
Totals	29	29	13

These findings may be compared with the ratings made independently by Dr X from interviews (of 1½-hr duration) conducted some six months after the previous assessments were recorded. Two ratings were abstracted from the data recorded by him—'symptomatic' improvement (relative to the symptoms for which the patient sought treatment) and 'over-all' improvement, from the total evidence of the interview. There is, of course, considerable overlap between the two assessments. The ratings were made on 7-point scales, though in fact no patients were given ratings of 5, 6, or 7. This may be due to two main factors. In the first place the latter

In Table 3 the number of cases placed in each category by Dr X is given ($n=29$). It will be seen that over half the patients (15 cases) show considerable or definite improvement. Almost 90% (26 cases) of the patients showed some degree of improvement (ratings 1 to 3). Dr Y rated the cases in respect of prognosis quite independently of Dr X, and 13 of the 29 cases were given a poor prognosis by him. It is of interest that seven of these 13 cases were rated as definitely improved after therapy (rating 2) by Dr X, and a further three cases were rated as slightly improved.

The assessments shown in Table 3 relate to a sample from 21 groups, the original dis-

tribution of age, sex and intelligence level being maintained as far as possible. Because of this, approximately half the patients came from groups on which assessments had been made by the doctors responsible for their treatment. To estimate the reliability of Dr X's ratings, the doctors (F to I) who had cases in this sample for which an assessment of change had not been made, were asked to make the relevant assessments of progress. Results were finally available for 28 cases, and the product-moment correlation coefficients between Dr X's rating and that of the therapist was calculated for 'symptomatic' and 'over-all' improvement. These were 0.480 and 0.413 respectively. These figures are misleading, however, if we consider that they do not take into account the agreement between the two sets of ratings on the *general improvement* of the whole group of 28 patients. In fact, the ratings for 'over-all' improvement were identical in 13 of the 28 cases, in a further 13 there was a discrepancy of one point of the scale only, and in the remaining two cases only was there a discrepancy of two points. The agreement on the scale of 'symptomatic' improvement was similar.

DISCUSSION AND CONCLUSIONS

Progress and severity of illness

From a scientific viewpoint there are a number of very justifiable criticisms which must be made about the method employed in this research. The criteria of change used in the study fall short of scientific requirements in several obvious respects. They are largely based on evidence provided by the patients themselves as seen and recorded by the therapist. It might be said, that they are dependent upon clinical judgements, and therefore, subjective and unreliable. However, a group therapy situation provides evidence of a different order, and opportunity for checking its consistency and validity, which is lacking in a two-person diagnostic or therapeutic relationship. The group provides a social situation in which the kind and

quality of relations with other people may be observed not only in the behaviour of the individuals, but also, and perhaps with greater validity, in the reactions of others to this behaviour.

It is well recognized that the problems concerning the reliability and validity of evidence for change cannot be solved without using techniques which provide an independent and external measure of changes in personality functioning, or without demonstrating that these changes are attributable to the therapeutic process rather than to other influences. It seems possible that the development of relevant objective techniques will be possible only on the basis of a fuller understanding of the therapeutic process than is at present available.

But in so far as the above results can be taken as an estimate of changes brought about by group psychotherapy (in co-operation with or in spite of external conditions obtaining in the patients' lives) they are encouraging. They are in keeping with the experience of the therapists concerned, and with the amount of effort and time given by the patients who persisted in this treatment over a prolonged period. The results are particularly encouraging in view of the increasing demands for out-patient psychotherapy in an expanding Mental Health Service, because two to four years of group psychotherapy on a once-weekly basis represents a considerable saving of time as compared with that normally required for individual treatment.

The amount of progress reported in this study would be less convincing if there had been any careful pre-selection of cases, so as to exclude those who were severely ill and who appeared to need individual help. In practice the degree of pre-selection on the grounds of unsuitability for group treatment in the clinic is low. It excludes only those who show at the diagnostic stage clear indications of imminent psychotic breakdown, and those patients who might be described as psychopaths with dependent and paranoid features. The latter type of patient often has a compulsion to act

out which might threaten the security of the treatment. Exclusion on any other grounds is not easily possible, partly because of the lack of any established criteria of suitability for group treatment, but also because of the small provisions in the Health Service for any other form of extended psychotherapy.

The independent ratings of original prognosis* show that a large proportion of the patients considered in this study were seriously disturbed, and the sample report given in the Appendix provides an illustration of the kind and degree of disturbance in family, work and social relations for which the patients sought help. In a large proportion of the severely disturbed cases well-evidenced progress is recorded. In many other cases attendance at the groups appears to have helped such very ill patients to continue to function, though still in an impaired fashion in the key areas of their lives.

The number of patients who broke down during group treatment is small (six out of 238 patients). These appear to fall into two groups:

(i) Patients who showed acute social anxiety, dependence and marked paranoid features, and who presented behaviour which was at times characterized by rigid control and inflexibility and at other times by impulsiveness.

(ii) Patients who again showed acute social anxiety, and severe hysterical behaviour.

There were three cases in each group, the common features being considerable immaturity and weak ego-resources, together with unusually severe emotional conflict and strong 'acting-out' tendencies.

More detailed examination of the records

* A bad prognosis was made on the basis of combinations of the following features: (1) a poor heredity; (2) a long-standing psychological illness; (3) lack of evidence of solid achievements in the past and of desire to change and mature; (4) a long-standing complaint about the poverty of environmental resources; (5) a monosymptomatic complaint with denial of any disturbances in other areas of life.

of individual cases included in this research shows that many of the patients who made definite or even considerable progress had symptoms as severe, or more severe, or were more incapacitated in key areas of their life, than some of those who broke down in treatment. They had also, however, a wider range of defensive resources and greater social skill in using them, and projective defences played a less dominant part in their personal relationships.

The composition of groups and the suitability of patients for group treatment

The general results reported in this study prompt questions concerning the nature of the therapeutic process in group treatment. In what important respects does it differ from individual psychotherapy? How may such differences account for the measure of success it appears to achieve and its economy of therapeutic time? These questions may be asked, for the present, without seeking to compare the kinds of personality change which the two forms of treatment are designed to bring about. Discussion with the group therapists concerned in this research has led to the hypothesis that the differences may result from the reality content of the social situation in which the therapy takes place. Related notions have been developed by the writer elsewhere (Phillipson, 1955, 1956).

A central aim of psychoanalytic therapy is to bring about an integration of unconscious perceptions and motivations with reality-based experience, and the main body of concepts which directs therapeutic action is in terms of interpersonal relationships; in particular, the interplay of unconscious phantasy relationships with those which have had their consistency and validity tested in conscious experience. In the therapeutic situation used in individual analysis, phantasy relations are intensified and the opportunity for reality-testing is restricted to the securities provided by the therapist. In the group therapy situation, there is a much greater balance of possibility for 'reality' experience because of the

face-to-face relation with the therapist and because of the presence of real people in the group. Consequently there is much greater opportunity for reality-testing while still making use of the securities which the therapeutic situation provides. In another way the presence of real people in that social situation, which is the context of therapy, probably provides greater securities for testing out the validity of unconscious hypotheses. On the one hand more direct defensive opportunities are available, particularly from visual cues, and on the other hand reassurances about the anxiety-laden consequences of phantasy wishes are also provided by the opportunity to observe the therapist and other members of the group directly. At the same time the group situation is directly evocative of social anxieties; and it may be that the combination of this feature with the security and opportunity for reality-testing is responsible for its success as a once-weekly form of therapy.

A rationale in terms of such concepts appears to fit the general findings in this study concerning the composition of therapeutic groups, and the kinds of patients who make progress in them.

Of the 15 groups on which a description of the main characteristics of the group process could be obtained (together with reports on their composition in respect of age, sex, marital status, intelligence, social and educational background, and nature and degree of illness) five were judged to be above average, seven as average and four as below average in efficiency of the therapeutic process.

No differences were found between groups containing patients of bright normal, superior or very superior levels of ability, or between groups whose members were all married and those in which there were varying proportions of married and single members.

None of the three one-sex groups was judged to be better than average. Of the three all-male groups reported on, two were considered as average and one below average.

There appears to be a tendency for the groups containing patients between 25 years

and 35 years to do better than those containing patients whose ages go well beyond 35 years, and better than those where the range of ages is more than 10 years. Of the eight groups in which the age range was approximately 25 years to 35 years, four are rated as above average groups and only one (which contained three pre-psychotic patients out of a membership of four) is rated below average. Of three groups whose age range is extended to 15 years or more, two are considered to be below average efficiency as therapeutic groups.

Examination of the groups in terms of the proportion of very severely ill, moderately ill, and less disturbed patients in them shows no differences between those rated above average, average or below in the therapeutic efficiency. The main differences which become apparent from the notes on the composition and working of the groups relate to factors which might be applied also to the individual patient in the groups as well as to the group as a whole. Stated in reference to the group they are:

- (i) The capacity of the group to develop a conscious recognition of its problems, and a consensus of opinion as to their nature, in terms of interpersonal relationships.
- (ii) Development of a recognition of some common problems, e.g. difficulties in marriage relations or heterosexual relations, or problems with parental figures.
- (iii) The over-all balance of ego-resources in the group as against the degree of disturbance.
- (iv) By the side of such resources in the group there is needed a factor which may be termed 'evocativeness', and which may be provided by all members together or at intervals by any individual member (one, for example, who is sufficiently secure to verbalize and to test out his phantasy relationships, or who expresses his problem with some degree of impulsiveness or acting out). In three out of five of the above-average groups the presence of

such a person was mentioned as giving considerable impetus to the progress of the group.

It also seems that a group which fulfils these positive conditions can carry and help one or two very ill members.

The reverse condition appeared to hold in those groups where the over-all ego-strength is weak, and also where the group is challenged by an impulsive acting-out member. An abundance of denial and projective defences (both of which are negations of reality content) was also mentioned in several instances as a considerable handicap to the group's progress.

It appears, therefore, from the data available in this study, that within very wide limits, the nature of the symptoms and the degree of illness are less important than the composition of the group as a whole, and the capacity of the patient to contribute purposefully to the group's aims in understanding problems in terms of relations with others.

APPENDIX

*The report form used by therapists to summarize evidence of changes in individual group patients**

Dr H. Group (a). Miss A.B. Age 26

Following the model worked out in discussion, and keeping in mind the standards arrived at on that occasion, make judgements on the patient's progress under the following heads, *providing evidence as far as is possible for these judgements*. A 7-point rating scale is to be used:

- 1. Considerably improved.
- 2. Definitely improved.
- 3. Some slight improvement.
- 4. No apparent improvement but appears to be getting something from the group.
- 5. No improvement.
- 6. Worse.
- 7. Breakdown.

In Section B a rating of severity of symptoms at the beginning of treatment is required on the 3-

* The ratings of an actual patient are given in heavy type.

point scale provided, and in Section C ratings of the extent of disturbance in relationships in 4 areas considered are to be given on a 4-point scale.

- 1. Very severe (incapacitating or nearly so).
- 2. Severe (marked difficulties).
- 3. Mild difficulties which do not seriously impair relations.
- 4. No real problem in this area.

A. OVERALL ASSESSMENT OF PROGRESS (do this after completing the assessments under B and C on following pages):

1 2 3 4 5 6 7

Reasons (draw attention to main points in evidence provided in B and C—very briefly).

(Removal of crippling phobic symptom has resulted in considerable change in social and work life, and has gone hand in hand with improvements in social and heterosexual relations.)

B. ASSESSMENT OF PROGRESS IN TERMS OF SYMPTOMS COMPLAINED OF BY THE PATIENT

(i) *Nature of symptom(s)*.

- | | |
|--------------------------------------|---|
| e.g. psychosomatic
phobic | (Agoraphobia, fear of going out alone; acute for two years, and unable to go from house alone.) |
| obsessional
depression
anxiety | (Fear of breaking down completely and being hospitalized.) |
| social
work
sexual | (Unable to work).
(Difficulties with men, self-conscious, unsure, not wanted.) |

(ii) *Rating of severity of symptom(s)*:

1. Very severe. 2. Moderately severe. 3. Mild.

(iii) *Evidence of changes during treatment*

(Had been one year as an in-patient with no change, and also had treatment at two other hospitals.)

(Can now go out alone: almost entirely free of this anxiety for past four months. Used to come to clinic in taxi as only way. Now comes freely and is not hampered in social and work life.)

(Fear of breaking down has now receded definitely to the background.)

(Relations with men better; has a boy friend and the relationship is maintained and is progressing—it is a neurotic sort of choice but she can feel she is wanted and is loved.)

(Applied for and obtained position as supervisor in a large departmental store. Held this successfully for about one year. Applied for and was accepted for course of study at a university. Started this work, successful and enjoying it.)

(Ceased to attend the group.)

(iv) *Rating of progress in terms of these symptoms:*

1 2 3 4 5 6 7

C. ASSESSMENT OF PROGRESS IN TERMS OF RELATIONSHIPS

(i) *Quality of relations in the group:* describe any changes with evidence.

(Definitely improved. At first was extremely anxious and not able to bring her feelings into her relations with others; can now do so to a considerable extent. Became much more relaxed and less self-conscious in making contributions in the group. Showed concern about others and their problems, which, though often expressed in a critical way, was felt by others to be genuine and helpful. Others in the group clearly recognized this new quality in her relations and responded to her in a way which showed they found her more interesting as a person, and someone for whom they had an increased respect.)

Rating of progress in terms of effectiveness of relations in group:

1 2 3 4 5 6 7
(Originally 1 2 3 4)

(ii) *Relations in work life and effectiveness in work:* describe any changes with evidence.

(Was at first unable to work. Has progressed from a very precarious adjustment in a subordinate job to one in which she is in charge

(supervisor in a large store) and more recently to a more responsible job in the same kind of work, the responsibility of which she could not have faced before. Finally accepted for a university course in which she was making good progress and was enjoying the work when she left the group.)

Rating of progress in terms of effectiveness in work and work relations:

1 2 3 4 5 6 7
(Originally 1 2 3 4)

(iii) *Heterosexual relations:* describe any changes with evidence.

More self-confident, less self-deprecating. Heterosexual relations are definitely better.) (See under Symptoms.)

Rating of progress in terms of quality of such close relations:

1 2 3 4 5 6 7
(Originally 1 2 3 4)

(iv) *General social relations:* describe any changes with evidence.

(Considerably better since loss of phobic symptoms. She has given many evidences of successful participation in many social activities, and this evidence has been supported by the observations made by other group members.)

Rating of progress in terms of range and quality of social relations:

1 2 3 4 5 6 7
(Originally 1 2 3 4)

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DEPRESSION IN GIRLS DURING LATENCY

BY MOLLY HARRINGTON* AND JANET W. M. HASSAN†

THE CLINICAL PICTURE

Of the fourteen female patients in the 8-11-year-old age group investigated and treated at this recently established Department during its first year, no less than half were found to be suffering from depression. The cases were referred in the following way:

1. Joan, aged 8. Weeping attacks and fears of death of parents.
2. Sally, aged 11, Hysterical outbursts and attacks of weeping.
3. Marie, aged 10. Fits of crying and diffuse pains.
4. Clarice, aged 9. Screaming attacks at night.
5. Marjorie, aged 10. Enuresis, school phobia and 'depressed anxiety'.
6. Hilda, aged 9. School phobia.
7. Mildred, aged 9. Severe headaches and vomiting.

Closer investigation established a common syndrome of weeping bouts, some flatness of affect, fears of death for self or parents, irritability, somatic complaints, loss of appetite and energy and varying degrees of difficulty in school adjustment. The onset in all cases was recent. Besides this common pattern of symptomatology striking similarities were observed in the girls' personality development and in their early life situations.

Up to the time of onset of their illness these had all been exceptionally 'good' children, rarely complained of either by parents or teachers. In their relations with adults they had continually to ingratiate themselves and propitiate authority. This being so, they were naturally considered to be helpful, kind and

sympathetic. There was in each one an over-valuation of success in schoolwork and ceaseless striving for recognition. Parallel with this investment in intellectual achievement and need to be recognized as clever good girls, ran a fear of failure and a belittling of their own abilities. All of them were of above average intelligence and four of them were exceptionally successful at school; it was in these four that the disparity between the reality of their continued success and their preoccupation with the likelihood of failure was most striking. As regards their social life, they were members of organizations and groups—as one would expect from such conforming girls—but it became clear on knowing them more intimately that their group identifications were superficial, their peer relationships unstable and marked by irritability which never reached overt aggression. None of them had ever had a 'best friend.'

Looking at closer family relationships, we found that the parents had hitherto been unaware of sibling jealousy and were puzzled by its sudden appearance at the onset of the disturbance. The relationships of the girls to their mothers will be discussed more fully in a later part of this paper. It is sufficient for the moment to say that these children related to their mothers as to other adults, i.e. with propitiation and absence of aggression. With one exception the mothers had not found the girls difficult in any way; the mother-child relationship was ostensibly wholly good until the breakdown occurred, when hostility began to show itself, being expressed as extreme irritability. In the mothers' descriptions, the phrase 'She is a "Daddy's girl"' recurred significantly, the only exception being Clarice who had a very positive relationship to her father until the age of four years when she apparently began to reject him. However, with

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the onset of her illness there was a reorientation of her relationship to her parents and she turned once more to her father after the pattern of the others. With all of the girls, the idealization of the good, loving father was displaced on to all men and they all looked forward eagerly to having male teachers, who would be equally kind and understanding. This change they regarded as a solution to their difficulties with women teachers. This, in brief, is an outline of the personalities and relationships of these girls up to the time of the depression. Certain traumatic events to be described later had put too great a load on their precarious adjustment, and the resultant breakdown revealed the underlying pathology.

At the time of the disturbance a number of changes occurred, the most significant and dramatic of which was in the relationship with the mother. In each case there was a swing between the extremes of very dependent clinging and unreasonable hostility. In some cases the period of clinging preceded the hostility, in others clinging followed the hostility and usually there was some overlap. In no case was the girl's behaviour a reaction to any change in the mother's attitude. The sibling rivalry which flared up had even greater repercussions on the family. The most extreme manifestations of this were seen in the four cases where the next youngest sibling was of the same sex. In general they ceased to conform to their idea of themselves as good girls and to the expectations of their teachers and parents. As might be expected, their teachers provided ready targets for the released hostility and the next step was a withdrawal from them in fear, with consequent failure in schoolwork and varying degrees of reluctance to attend. This retreat was inevitably extended to other outside activities and they refused to participate in such things as Sunday Schools, Guides, dancing classes, and in so doing they also severed their already tenuous group identifications and friendships. The further purpose of this withdrawal to the shelter of the home was to reassure themselves constantly by the mother's presence that their hostile phan-

tasies had not harmed her. One thing only remained unchanged: the very positive relationship to the father. If anything, the tie was intensified and in the one case where it had been repressed, the crisis saw its re-emergence.

These changes of personality were all very recent and could be related with some certainty by the parents to specific events. These were accidents and physical illnesses. For instance, Marie's upset followed excited talk amongst schoolfellows of disablement resulting from poliomyelitis during a current 'polio scare' in the community. Sally fell whilst on a forbidden expedition. Her side was pierced by an iron spike and the wound required stitches. Her reaction was intensified by a minor accident to her baby sister when left in her care. The case of Marjorie was very similar in that one trauma followed another. She was upset by witnessing two very gory and fatal street accidents and was involved, along with other schoolgirls in the teasing of a queer character in the neighbourhood which resulted in his threatening her with scissors. To give another example in rather more detail Hilda can be cited. Here the disturbance began with her admission to hospital for a possible appendicectomy. She could recall her fears at the initial examination in the ward when a coloured doctor asked her to put her leg out of bed; a baby with 'wild teeth' lay in the next cot, and throughout her 24 hours' stay in hospital she stared in horror at this child's bleeding mouth and the tube through which it had to be fed. The climax was the ward-round when she heard her case discussed by a group of doctors, one giving his opinion that she ought not to have been admitted whilst another disagreed saying, 'I would take a chance and cut'.

Alongside these illnesses and accidents, events in school also played their part. Five of the girls were afraid of rather punitive women teachers, although as good pupils they had never invoked disapproval or punishment themselves, and this fear contributed to the difficulty in attending school. In some cases there were weeping outbursts before leaving home, in others minor somatic symptoms

were complained of, and short absences from school were frequent. In two cases only was there a frank school phobia, one girl being unable to attend for three months and another having been off for six months up to the time of writing.

It will be agreed that such stresses as we have mentioned are not in themselves overwhelming but are such as most children encounter and survive without breakdown. Wherein then lies the weakness in the child who succumbs to them after years of apparently good adjustment? We may also ask why the illness took the form of a depression. Much has been made in discussion of depression of the loss of a loved person as the main causal event. In these cases this hardly entered into the picture, since in only one instance was there any loss at all and that not very important to the child in the hierarchy of trigger factors. In the following section an attempt is made to answer the two questions which have been raised.

AETIOLOGICAL CONSIDERATIONS

Of the various phenomena associated with depression, that of self-depreciation particularly characterizes these girls. Indeed, it was so noticeable that we felt the necessity to trace its origins and the ensuing investigations have served to emphasise further its importance in such conditions. The work of Edward Glover (1949) and Edith Jacobson (1955) have been found to be specially helpful in the consideration of our cases.

Most writers on ego psychology are in broad agreement that the ego is formed in the first place out of mental representations of the gratifying and ungratifying mother. With increasing awareness of the self as distinct from the outside world, there arises also a mental representation of the self formed out of body part-images, which is co-existent with the representation of the mother but separate. Maturation gives stability to this mental structure, with increasing amounts of libido invested in the self, but it is obvious that for frequent periods in early childhood

there will be felt a need to regress to a more primitive state of oneness with the mother, which will bring about a reinforcement of primary identification. During these times the tentative boundaries of the ego nuclei are weakened so that both the self-image and that of the mother are simultaneously flooded with whatever affect prevails at the time. The recurrence of this phenomenon hinders and postpones the final separating-out of the internalized images of self and object, a state of mutability which characterises early childhood, and if this state of mutability is unduly prolonged it must surely prejudice the development of a strong ego. This maturational lag can be related to a number of life situations, some of which are demonstrated very clearly in the histories of these girls.

Meanwhile, let us look at the origins of self-depreciation, to the importance of which, along with ego weakness, we have drawn attention. One of the earliest responses of the infant to what is unpleasurable is the physiological one of spitting out, a response which later becomes the feeling of disgust accompanied by the discharge of aggression. As object relations are established such aggression is directed towards the mother at times of frustration when she is seen as 'bad'. All the necessary frustrations in infancy and early childhood call forth this aggressive-derogatory response, and it is essential for good personality development that this be countered by a sufficiency of libido invested in the mother, gradually leading to a tolerance of ambivalence, with positive feelings predominating. To return to the concept of re-fusion of self and object: at such times the aggressive-derogatory feelings cannot be directed against the mother without overwhelming the self. Again, if there is insufficient libidinal investment in the mother-image there will be a libidinal impoverishment of the self-image. The self-image being perhaps the most important of the ego nuclei, an ego thus weakened will be particularly vulnerable in ensuing conflicts with the super ego.

To return to the questions of the 'why' and

the 'how' of these depressive breakdowns, we believe the 'why' is accounted for by ego weakness and the 'how' by this tendency to self-depreciation at the very root of ego structure.

It remains to examine the life situations in which such a combination of ego weakness and self-depreciation has its origin, and in the life situations of the subjects of our study certain common features emerge. Three out of the seven have lived with their parents in the home of grandparents during the first two to four years and in a fourth case the matriarchal nature of the mother's family made the grandmother a more than usually important figure in the child's life. In all these cases maternal aunts were also in the picture and both grandmothers and aunts took over some functions of mothering. The following brief outlines illustrate these factors:

(a) *Joan*. The parents lived in the maternal grandparent's home (which included a maternal aunt) until the child was nearly five. Grandmother took over whole areas of the care of the child, particularly those functions providing gratification. Joan was always more spontaneously affectionate towards her than towards her own mother. Moreover, the father in this family is a very indulgent and permissive man.

(b) *Sally*. This mother and child also lived with maternal grandparents and aunts for three years whilst the father was in the Forces, although he was a frequent visitor on leave. This girl has always maintained a strong attachment to her grandmother but since the onset of her illness this has been exaggerated to a degree which has caused inter-family strife. Sally has acted for the last 18 months as if grandmother were the wholly good figure and mother the wholly bad.

(c) *Clarice*. Again, parents and child lived with maternal grandparents and aunts during the first two years. According to the mother, both grandmother and aunts were far more indulgent than she was as regards lifting, nursing, comforting, etc. Clarice reacted with a sleep disturbance immediately the parents moved into their own home.

(d) *Hilda*. Her parents did not live with the grandparents but the grandmother and several aunts have played almost as big a part in her life as

the preceding cases and have encroached on the mother's role in the same way.

We believe that this multiplication of mother-images withdraws from the real mother—who is necessarily a frustrator—large sums of available libido on to the substitutes. It would seem inevitable that, when mothering is split (i.e. when situations arousing aggression such as oral frustration and toilet training are in the hands of the real mother whilst comforting and indulgence come from the substitutes) the internalized image of the real mother will be disproportionately weighted on the aggressive side. The libido which should balance this aggression and make possible the eventual toleration of ambivalence is drained off by these other women. If this situation is perpetuated throughout childhood the girl is likely to keep the mother-images separate and will express overtly her hostility to her own mother; but in the cases under discussion it can be seen what happens when the family become a separate unit again and the child is thrown back on her own mother for the satisfaction of all her needs: she must repress much of her hostility. It was this repressed hostility which emerged with the break in defences when the disturbance began.

We have however to account for the fact that although in the remaining three cases there were no multiple mother-figures involved, there was still ego weakness and self-depreciation present. What was found amongst these three was a striking similarity in the father-mother-child relationship. In the early years the fathers had taken over the mothers' roles to some extent and with the same result, namely a withdrawing of libido from the mother-image. Our assessment of this situation was made not only from the mothers' stories that the fathers had done a great deal more in the way of feeding, bathing, etc., than is usual in the community to which they belong, but from the gradual coalescence of items of evidence given both by mothers and daughters during treatment. As seen by them.

the fathers were all very maternal men who even at the time of referral were protective and permissive. Their role was not unlike that of the grandmothers in the four cases previously described. In fact, this personality trait was a feature of all the fathers of all seven children with one exception; Clarice's father who has been a disciplinarian to his daughters as they grew older. She has therefore been able to use him as a compensatory mother-figure less and less as time went on. Her earlier relationship to him was relived in a vivid phantasy told to the therapist in the third treatment interview, where she and her father went through the woods to an open space where a certain Christmas tree grew and from it he gave her boxes of chocolates and sweets; this would not have happened if her mother were present. Other evidence in therapy supports the view that not only has this phantasy its obvious Oedipal connotation, but underlying it is the significance of the father as satisfier of oral needs.

We have up to now tried to demonstrate the connexion on the one hand between split mother-image and ego weakness, and on the other between repressed hostility to the insufficiently libidized mother-image and depreciation of the self. But this opens up questions about further emotional development, questions relating to feminine identification, superego formation, and the later development of the ego ideal. In these cases—as in others where parent-images are invested with unneutralized aggression—the superego nuclei are so punitive as to drive the weakened ego to continued use of projective defences in its attempts to defend itself. The issue which throws this system into relief is, in normal development, the first recognition of sex differences, a crisis which leads to a more or less transitory depreciation of herself. She attempts to deny her deficiency and phantasies the recovery of the lost penis. Moreover, in so far as her mother is also feminine she depreciates her too and rejects her as a love object in favour of the father. To support her through the inevitable strains and frustrations

at this stage the girl requires above all the nucleus of a strongly feminine ideal. This ego ideal, which arises out of the more positive aspects of the identification with the mother and which can be regarded as a libidinal cathexis of part of the superego, is for the reasons suggested, conspicuously lacking in the cases under discussion. Instead, there was an overvaluation of the phallus representing the good, kind father and an ego ideal predominantly masculine which augmented their difficulty in accepting femininity. Further, this ego ideal was and is attached to the real, overvalued father, therefore it remains sexualized and external and cannot strengthen the ego and support self-esteem but leaves it continuously dependent on external support. For the foregoing reasons girls such as these are unable to resolve the Oedipal situation and to work out the problems of latency, continuing with unsuccessful attempts at identification. Such identifications are unstable since the original one with the mother was not satisfactorily established. During latency therefore an uneasy relationship with sexualized objects will be maintained, and it follows that any situation which disturbs this precarious state will undermine the defences of the weakened ego.

To return to the significance of the symptoms in these depressive states, it might be thought that the self-depreciation could be accounted for by castration anxiety, particularly as fears of death and injury were so prevalent, and as accidents and illnesses were prominent amongst the trigger factors. There was, however, no evidence in therapy that such death fears could be equated with castration fears, and we believe we are nearer the truth in tracing the origins of self-depreciation back to this earlier phase of ego and superego development. These death fears were in fact manifestations of hostile impulses towards the mothers, whether the death feared was that of the mother or that of the child herself.

Before leaving the discussion of aetiology we must consider one aspect which has not

yet been more than mentioned in passing—the mother-child relationship. No study of any childhood disorder can leave this out of account, in particular one in which the role of mother-substitutes is emphasized. It may be said that the very fact that maternal relatives played such an important part in the children's upbringing points to the existence of difficulties in the mother-grandmother relationship which might inevitably be relived in the mother-daughter relationship. To our knowledge, this only happened in one case, and the disturbed mother-child relationship (which responded to treatment) could plainly be related to certain personality traits and behaviour. With the other three cases where the maternal grandmother had played a part, the mothers themselves were still immature and tied at a very deep level to the grandmothers and neither overtly dependent nor overtly rebellious. They had adapted very well to all circumstances, and the only clue to their arrested personality development was the mother-child relationship which develops with the years into one of older sister to child. None of these mothers were without warmth and none was so immature as to be unable to give support. As far as those cases were concerned where there were no multiple mother-figures, but where the fathers played a maternal role, the mother-child relationships showed no distortion nor did this sibling relationship with their daughters arise. These three mothers had much in common. They were women who were very devoted to their families, who had themselves been deprived of good mothering in childhood and who had not fully met their girls' dependency needs in the early years but had expected behaviour which was a little in advance of their stage of development. They had felt under the necessity of doing so because each of these girls was quickly followed by another sibling and the girls had played into these expectations by being intelligent and purposeful toddlers. The mothers all had some degree of difficulty in tolerating aggression but in treatment they were all, without exception

able to meet the children's needs not only for dependency but also for self-assertion. It is interesting to note that although the breakthrough of hostility to the mother occurred in all seven cases, it took a rather milder form in the subgroup last referred to than in the one where mother-substitutes existed.

Taking the group of seven as a whole, the child who was most severely depressed showed all the phenomena described in the most dramatic form. Clarice had experienced the most extreme splitting in the mothering functions and she reacted to separation from her grandmother with the most disturbance. Of all the mothers hers is the one most afraid of aggression and the one who has set the most store by achievement; at the same time this mother-child relationship most resembles that of older and younger sisters. The child expressed it herself very neatly when reprimanded for 'cheek' by saying: 'Sorry mum, I forget that you're not one of my chums.' It was also Clarice, it may be recalled, who was unable to use her father as a compensatory mother-figure after about four years of age. She showed most strongly such features as unprecedented sibling rivalry and fear of female teachers, and was the most clear-cut example of the personality pattern outlined earlier in this paper. Conversely, the child whose illness was the mildest and shortest-lived was one of the group with no plurality of mother-figures and whose own mother could be rated the most permissive.

It may be asked if there was evidence of depression in any of the mothers or if there was any such loading in the family histories. There was no known case of depression in the families. The father of one of the girls was a schizophrenic of three years standing, whose affection for and interest in his children has remained unimpaired; apart from that there are no family histories of psychosis or recognized neurosis. None of the mothers were or had been depressed and only one had ever had symptoms—these being a mild hysterical type. No emotional disturbance was ever reported in any of the siblings.

IMPLICATIONS FOR THERAPY

Certain aspects of treatment highlight the pathology of ego structure. The release and acceptance of hostility to the mother was the main therapeutic achievement. This hostility had hitherto been dealt with by a combination of defence mechanisms, mainly repression and reaction-formation. Aggressive and sexual impulses had been transformed into bodily symptoms, and reaction-formations of excessive tenderness, unselfishness and thoughtfulness were used extensively as checks upon any manifestation of these impulses. Therapy had to be directed in the first instance to the interpretation of these resistances, a process which necessarily provoked aggression which, in its turn, could more easily be tolerated thereafter in relation to the mother. Following these interpretations there was a period of regression to very dependent clinging to the mother, a development which was met with understanding, following insights gained in casework. This infantile clinging, being a true regression, was of a different nature to the separation-anxiety which they had shown at the onset of the illness.

In the treatment process the chief technical problem was their too ready use of identification. This difficulty was demonstrated most convincingly by one of the girls in a therapeutic group—she was the only one who was treated in this way, the rest having individual sessions of interpretive play therapy. She could only make a relationship on the primitive level of imitation and this she carried to the extent of walking around her friend, aping gestures, expression, gait and actions, unable to hold on to any identity of her own until she was established in a secure relationship. Although not always to this degree, the misuse of identification was more characteristic where multiple mother-figures had hindered the formation of one strong initial identification upon which later relationships could be based. The effect of such failure is that in therapy the therapist is added to the constellation of mother figures. Their extreme

suggestibility stems from their wish to be the therapist rather than to use her, thus providing yet another example of the need of such weakened egos to re-fuse with introjected parent-figures. Of the children with this particular difficulty, Joan had the most intensive and successful treatment. The relationship with the therapist did permit her to play out and accept interpretation of her phantasies, which very gradually became Oedipal in character, and the working-through of these phantasies allowed her to begin true latency—Joan was eight years old at the time of referral. In contrast, Marie was at the age of ten years when Oedipal phantasies are least accessible and in her case the resolution of the Oedipus complex must await puberty. She was a very intelligent girl with strong emotional drives and outstanding artistic gifts. With the acceptance of hostile feelings and fears in relation to her mother the resultant freed libido found ready sublimation in her enjoyment of and skill in painting.

The kind of therapeutic success achieved in the last-mentioned case led to a reappraisal of the prognosis for girls with such psycho-sexual difficulties. It seems reasonable to hope that their continuing tendency to identify might enable them at puberty to reinforce their feminine identifications and consequently to resolve the Oedipal situation. This would be less likely to be achieved, however, as long as the hostility to the mother continued to be bound within such feminine identifications, and we have concluded therefore that therapy with such girls in later latency is in the nature of a preparation for the work of puberty by freeing of libido. Follow-up studies have shown that some measure of ego weakness will still persist in spite of freedom from symptoms and genuine all-round emotional adjustment. For example, in Joan there is still too great a sensitivity to her mother's moods and feelings and with Marie the tendency to self-depreciation underlies her drive for achievement, although it is no longer fed by repressed hostility and is offset by social approval of her many gifts.

We have used the term 'depression' freely in this discussion but it should be understood that something other than a psychotic depression is in question. There was no cyclothymia in these girls and at their most disturbed they showed no delusions of guilt or suicidal tendencies, nor was there any retardation of thought. In all, their reality testing was satisfactory throughout the illness. In contrast to the very low incidence of psychotic depression in children the incidence of this syndrome here described (which more resembles a neurosis) must be comparatively high and it is therefore the more surprising that so little has been written on the subject, although several instances appear in cases quoted of school phobia who would appear to justify a diagnosis of depression. It is tempting to link these episodes with the very common mild recurrent depressions in adult women; but for this to be justified one would need either a follow-up of such children into adult

life or detailed histories of such adult patients in the first years of life.

SUMMARY

This is a study of seven girls aged eight to eleven years who were found to be suffering from depression. The ego weakness and self-depreciation present in all cases were found to be of the greatest significance. An attempt has been made to relate such ego weakness and self-depreciation to faults in early identifications. A splitting of the function of mothering in the first two years of life had hindered the formation of strong feminine identifications. Such weakness in ego structure has been related to problems in therapy.

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PROBLEMS OF T.A.T. INTERPRETATION AND THE DIAGNOSIS OF DELINQUENT TRENDS

BY J. G. LYLE AND A. A. GILCHRIST*

The purpose of this study is primarily to clarify methods of interpreting the Thematic Apperception Test (T.A.T.). The complex nature of T.A.T. data makes a subjective approach to interpretation inevitable, for there is no scoring system and no hard and fast method of interpretation. A major problem therefore is that the clinician may often not be sure if his interpretation truly reflects the problems and phantasies of the patient. This uncertainty can only be overcome by the development of psychological theory to the point where the phantasies, object relations and intrapsychic reactions of the various clinical syndromes can, in a general way, be predicted; and by developing and validating principles of T.A.T. interpretation based on such predictions.

The authors have been particularly concerned with problems of delinquent behaviour, and have for some time included the T.A.T. in their regular test battery. However, some doubt was felt as to whether the interpretation of underlying conflicts and phantasies derived from this technique may not be equally applied to non-delinquent subjects.

Perhaps the most notable study concerning the projective testing of delinquents was that of the Schachtels, whose criteria of delinquency on the Rorschach were used by Glueck & Glueck (1950) in their *delinquency prediction scale*. It is noteworthy, however, that these predictive criteria were derived more from the testee's approach to the test and his acceptance of the instructions than from an orthodox analysis of the profile and content. This is because problems of delin-

quency, by their nature, are more specifically detectable in interpersonal situations than in perceptual techniques such as the Rorschach.

The T.A.T. is peculiarly suited to the analysis of phantasy material and conflicts concerned with interpersonal relations. If delinquents differ from non-delinquents in basic attitudes and conflicts in their interpersonal relations, it can be expected that the T.A.T. will systematically differentiate between delinquent and non-delinquent groups.

A secondary purpose of this study is then to discover whether the known behaviour of delinquents is reflected in thematic material.

The population

The delinquent sample consisted of 31 boys, all of whom had recently been charged with offences at a children's court. There was no special selection of cases by the psychologists for this study, six T.A.T. cards being administered to offenders whenever time permitted. Only one of the delinquents was considered to be potentially psychotic and a small number had minor neurotic symptoms (e.g. enuresis). But although it is true that delinquents may be classified into several subgroups, it is also true that there is much overlap between the subgroups. The typical 'antisocial syndrome' was defined as follows:

(a) Unstable behaviour, e.g. wandering from home, truancy, poor motivation at school, unstable job record, inability to maintain interest or motivation in long-term goals, suggestibility ('easily led', chooses antisocial cliques), irresponsibility, hedonism, etc.

(b) An inability to tolerate restriction or supervision, or to conform to authority (sometimes uncontrollable at home).

* Psychologists in the Department of Mental Hygiene, Victoria, Australia. Manuscript received 8 February 1957.

(c) Antisocial behaviour, e.g. petty stealing, frequent lying and deceit, sometimes vandalism and minor sex offences.

Of course, all delinquents do not show *all* these tendencies, but most show several of them, for delinquency-prediction tables can be based on such data. This was certainly the case for our sample. The individuals varied in behaviour at the Clinic from submissiveness, co-operativeness to passive defiance; at home their behaviour varied from 'obedient and thoughtful' to 'nearly out of control'. But the sample is considered homogeneous in that all our cases conform to the above syndrome, and that all had committed a similar type of offence at least once previously.

The non-delinquent sample consisted of 36 males from two classes in a technical school, situated in a predominantly working-class area which is officially recognized as one of the 'delinquency areas' of Melbourne.

consider it as an attempt to clarify methods of T.A.T. interpretation in the light of known behavioural differences between the two groups.

Description of cards used in the study

Six cards from the Murray T.A.T. were administered to both groups. The cards used were 3 BM, 4, 6 GF, 12 M, 13 MF and 18 GF. Selection of these cards was not based on any empirical nor on any theoretical consideration relevant to this study. It merely happened that these cards had been administered to a number of delinquents for another purpose.

THE CLASSIFICATION OF THEMES EXPERIMENT

Reports in the literature suggest that the following traits are frequently found in groups of delinquent males: assertiveness, destructiveness (Glueck and Glueck, 1950), extraversion (Eysenck, 1955), extrapunitive (Grygier,

Table 1

	Delinquent sample	Non-delinquent sample
Number in sample (males)	31	36
Mean age (years)	14.4	13.6
S.D. (years)	1.4	0.6
Mean I.Q.	93.8	100.8
S.D.	13.4	11.0
Father's occupation		
Unskilled	10	9
Semi-skilled	4	6
Skilled	8	10
Commercial and small business	2	8
Professional	0	2
Father dead or absent	7	1
Type of offence		
Larceny	28	0
Breaking and entering		
Illegal use of car		
Sex offences	3	0

Table 1 shows that the delinquent sample has the expected bias (Barry, Stoller & Barrett, 1956) towards lower socio-economic class, lower I.Q. and absence or death of parent. This bias should not invalidate the study if we

1954), unstable behaviour (frequent change of job, truancy, etc.), and antisocial behaviour (Barry *et al.* 1956). These characteristics are indeed commonly observed in delinquents, and certainly in the delinquents of this study.

It is generally assumed by those psychologists who insist that T.A.T. validity depends upon a compilation of norms, that a classification of themes would reflect the behaviour of the subject. If this assumption were true, it might be expected that themes involving the acting-out of aggression, feelings of aggression and destructive and antisocial behaviour, should occur more frequently in a delinquent than in a non-delinquent group.

To test this hypothesis, the T.A.T. stories of 31 delinquents and 31 unselected non-delinquents were classified according to their themes using the six cards. The results are given in Table 2.

teristic of non-delinquents, since this would be an important motive for conforming to parental norms. Delinquents often have inadequate object relations, and this may result in feelings of being 'sick'; i.e. of being in need of love and support.

These findings would not have been postulated on the basis of manifest behaviour. This type of analysis therefore fails to support the hypothesis that delinquents would give more aggressive, antisocial themes than non-delinquents in their T.A.T. protocols. But it should not yet be assumed that the T.A.T. does not reflect the known behaviour problems of delinquents. After all, interpretation of

Table 2

Classification of theme	Delinquents (N=31)		Non-delinquents (N=31)	
	No. of themes	No. of persons giving themes	No. of themes	No. of persons giving themes
Aggression + (feelings of aggression and quarrels)	20	15	14	10
++ (fighting)	14	7	10	5
+++ (murder and attempted murder)	43	21	45	25
Antisocial acts	18	11	17	11
Loss of loved object	19	5*	36	19*
Rejection, persecution, punishment	9	7	10	9
Suicide	2	2	7	5
Accidental death	7	4	6	6
Sick, injured	33	23*	18	14*
Love, loyalty, achievement	13	11	18	14

In this analysis, significant differences (by two-tailed χ^2) occur between the number of delinquents and non-delinquents for each of the following themes: 'loss of loved object' (significant beyond 1% level), and 'sick, injured' (significant at 2% level). No significant differences occur between delinquents and non-delinquents for any of the other items.

These findings are difficult to interpret. It might be that the delinquent is less able to tolerate the thought of 'loss of loved object' than is the non-delinquent. More likely, the concern with the loved object is more a charac-

phantasies and defence mechanisms, not classification of themes, is the main method used by clinical psychologists in analysing the T.A.T.

The type of story will, of course, be partly a function of the stimulus material (e.g. card 13MF will yield themes mostly concerned with sickness or murder), but an analysis which aims at a knowledge of the individual's phantasy life, his conflicts and defences, must concentrate on that part of the story contributed by the *person*. In short, it is hypothesized that not the type of story, but an interpretation of the organization of the story,

will be most revealing of an individual's problems.

SUBJECTIVE SELECTION EXPERIMENT

Since a classification of themes had not differentiated in the expected manner between delinquent and non-delinquent groups, the question remained as to whether the T.A.T. could be used for this purpose. To determine this, three psychologists* were given the task of distinguishing delinquent and non-delinquent protocols (six stories per protocol). The psychologists were not allowed to see the protocols, which were read aloud to them by a fourth psychologist.*

Each psychologist formulated criteria for his choice on an *a priori* basis, and applied them to each story in the 30 delinquent and 28 non-delinquent† protocols which were read out. One, who lacked experience with delinquents, failed to differentiate much better than chance. The other two, who were well experienced with delinquents, differentiated significantly better than chance (significant beyond the 1% level with two-tailed χ^2). Their results are given in Table 3.

Table 3

	Psychologist A		Psychologist B	
	Delinquent	Non-delinquent	Delinquent	Non-delinquent
Guessed delinquent	22	6	20	4
Guessed non-delinquent	8	22	10	24
	30	28	30	28

Comparison of the criteria used by the two successful psychologists showed some differences, one having concentrated on criteria for delinquency and on criteria for determining the *identificant* in each story; while the other had elaborated separate criteria for delinquency and non-delinquency.

* We wish to acknowledge the help of Mr L. Groh and Miss N. Scott, of the Department of Mental Hygiene, Victoria, in this connexion.

† All the protocols were not read out because of a technical error.

Pooling of criteria

It was decided to pool the criteria of the two successful psychologists in an attempt to determine the value of each of the criterion items. The criteria for determining the *identificant* in each story were the same as those originally formulated by psychologist B.

Criteria for identificant. The *identificant* is the character in each story to whom feelings are most attributed, whose needs are most elaborated, from whose point of view the action takes place, or whose description is most realistic. Where a character is 'stagey' or fantastic, such as a marauding burglar, a madman at large, or a priest or policeman, the identification is usually with a more realistic person in the story.

It is important to remember that only the behaviour of the *identificant* has been classified as either delinquent or non-delinquent.

Delinquent criteria. The delinquent criteria for classifying the *identificant's* behaviour were as follows:

(1) Aggressive identifications without guilt-avoidant mechanisms or preoccupation with

guilt. (Where fear of consequences was emphasized this was not considered to be guilt.)

(a) Aggressive feelings not translated into action.

(b) Aggressive destructive behaviour.

(2) Identification with unstable characters:

(a) Defies authority; won't conform: leaves, or remains outside the family circle.

(b) Unstable relationships with others.

(c) *Identificant* is double-crossed or fears being double-crossed.

(3) Antisocial or asocial identifications *without* guilt-avoidant mechanisms or preoccupation with guilt.

(a) Stealing, bribery and other anti-social identifications.

(b) Hedonistic identifications, preoccupation with money (money = love).

Non-delinquent criteria. The non-delinquent criteria used in classifying the identificant's behaviour were as follows:

(1) Aggressive or antisocial identifications *with* guilt-avoidant mechanisms or preoccupation with guilt.

(a) Distance placed between self and aggressive anti-social conduct, e.g. distance in place and time, naming of characters, dramatizing (characters are 'unreal' as if in a film or novel).

(b) Extenuating circumstances for behaviour; e.g. forced by others; or accidental, temporary loss of ego-control (drunk, insane); moral justification for behaviour.

(2) Guilt reactions; e.g. fate intervenes to prevent crime, gives self up or commits suicide (not merely through fear of consequences). Emphasis on guilt.

(3) Identification with stable characters; family cohesion; stable relationships emphasized.

(4) Identification with social values.

(a) Social or moral values emphasized; does 'right'; virtuously overcomes temptations; makes guilty reparation, etc.

(b) Social achievement; meritorious acts or goals.

These were 'conflict or difficulty in controlling own aggression' and 'domestic conflict'.

One criterion was added to the non-delinquent group after the analysis: 'temporary loss of ego-control'. (All the others had been formulated *a priori*.)

Application of pooled criteria to data. A score was compiled using the pooled criteria, and protocols were interpreted rigorously according to these criteria. All the work was double-checked and the few differences in scoring were discussed. Each of the six stories for each subject was then interpreted as conforming to delinquent or non-delinquent criteria. (In the relatively few cases where both delinquent and non-delinquent criteria apply to a story, the story was classified according to where the chief emphasis lay. Many of the criteria, however, are mutually exclusive, e.g. 'aggressive destructive behaviour without guilt-avoidant mechanisms' cannot be also classified as 'aggressive destructive behaviour with guilt-avoidant mechanisms'.) A score for each protocol was obtained by subtracting the number of stories classified as non-delinquent from the number classified delinquent ($D - N$). The most satisfactory critical score for delinquency was discovered to be $D - N > \text{zero}$ (i.e. for scores of +1 or more the protocol was classified as 'delinquent'). Using this critical point, the pattern of scores for the delinquent and non-delinquent groups is as shown in Table 4.

There are therefore striking differences in score for delinquent and non-delinquent protocols.

Table 4

	Delinquent	Non-delinquent	
Classified delinquent	26	2	28
Classified non-delinquent	5	34	39
	31	36	

Other criteria later reclassified. A few of the criteria originally included were later reclassified under the criteria described above because of the small number of themes involved.

When the items are considered individually, using χ^2 to test their significance, it appears that one delinquent item 3(b)—hedonism—does not differentiate between the two groups.

One non-delinquent item 2—stability—does not differentiate. The remaining items attain a high level of significance. Two-tailed χ^2 was used throughout, with Yates's correction where indicated.

The three subgroups of the non-delinquent item 1(b)—extenuating circumstances—were combined for the reason that the individual sub-groups were too small for meaningful results, and on the ground that they are all logically related. The two subgroups of the non-delinquent item 2—guilt reactions—were combined for the same reasons. The two subgroups of non-delinquent item 4—social identifications—were combined.

Psychological significance of the criteria

It is clear that the delinquent and the non-delinquent items are opposites, and therefore mutually exclusive. Parental and social values have been internalized by the non-delinquent, whereas the delinquent expresses unstable relationships and antisocial impulses. The expression of aggressive or antisocial impulses in phantasy arouses various guilt-motivated defences in the non-delinquent, but not in the delinquent. These differences were hypothesized on an *a priori* basis by the authors.

An interpretation of these non-delinquent guilt-motivated defences now needs to be made:

(1) *Distance between self and aggressive, anti-social identification*

(a) (i) Distance in time and space, e.g. placing the action of the story in a distance country or era (e.g. 'this happened in America' or 'this happened long ago'). By this device, subject stresses that *he* could not possibly be involved in such a story.

(ii) Distance by naming the characters (e.g. 'As Roger was walking home he met Jane...'). By this device subject stresses 'It isn't about me, but about somebody else.'

(iii) Distance by dramatizing the story, so that it seems like an excerpt from a novel or essay (e.g. 'Curse it!' he muttered, as he fired

and missed.' 'The Arrow gang closed in relentlessly.') By this device, subject stresses: 'It isn't true, it's only a story.'

(b) Extenuating circumstances for aggressive antisocial behaviour.

(i) Forced by others; accidental (e.g. man is hypnotized to commit a crime, or black-mailed, or forced by threat). By this device subject stresses that he would not voluntarily commit such an act.

(ii) Temporary loss of ego control (e.g. man commits murder while drunk or insane and is usually penitent afterwards. (*Note.* Ascertain that subject is really identifying with such a character, who should not be merely part of the *stage effect*, and incidental to the story.) Here again, subject stresses that he would not voluntarily commit such an act, i.e. not unless he were out of his mind.

(iii) Moral justification (e.g. man steals to save sick daughter or kills to put sick wife out of agony, etc. By this device, subject emphasizes that he would never commit any legally wrong act, unless for the highest possible motives. Such characters are sometimes endowed with extra morality by describing them as judges, missionaries, etc.

(2) *Guilt reactions*

The two subcategories indicate a fear of a stern punitive superego. In one instance it is projected as Fate, which will surely prevent and punish wrong-doing. In the other instance the superego demands self-punishment.

Of the *delinquent criteria*, only one seems to need explanation, the fear of being double-crossed. Identificant's wife, friend, or partner in crime gives him up to the police, steals his money, or injures him in some way so as to further own ends (e.g. wife has other boyfriend. Girl-friend rings police to inform them of his crimes. Partner in crime knocks him out and takes all the ill-gotten gains, etc.).

This item is clearly a special aspect of 2(b) 'unstable relationships'. In this latter category, subject reveals his *own* instability, infidelity and failure to make lasting relationships; while in 2(c)—fear of double-cross—he

feels that trusted friends will forsake and deceive *him*.

Possible objections

Analysis of the data revealed that naming and dramatizing were particularly important mechanisms used by the non-delinquent group. A possible objection is that this mechanism might be an artefact of educational differences between the delinquent and non-delinquent groups. To study the seriousness of this criticism, the non-delinquent group was split into two, one subgroup composed of those who used naming and dramatizing, and the other subgroup composed of those who did not. (*Drama* subgroup and *no-drama* subgroup.) It was found that the no-drama subgroup could be differentiated from the delinquent group could to the same extent as the non-delinquent group as a whole.

The next procedure was to study the effect of ignoring the criterion 'naming and dramatizing'. The result was to change the classification of protocols from non-delinquent to delinquent in only three cases. A highly significant differentiation (beyond 1% level) between delinquent and non-delinquent groups was still obtained in spite of ignoring this criterion. Where naming and dramatizing is used the story is usually classifiable under other non-delinquent criteria.

(2) Classify the behaviour of the identificant in each story under the delinquent or non-delinquent categories previously tabulated (often more than one delinquent or non-delinquent category is applicable to the one story; but generally it is not possible to classify a story both as delinquent and non-delinquent). If the story cannot be classified according to the given categories, leave it unclassified.

(3) Total the number of delinquent and non-delinquent stories for the six T.A.T. cards, ignoring Unclassified stories. If the delinquent outnumber the non-delinquent stories, the testee is considered to show delinquent trends (i.e. an unstable person who is likely to act out his antisocial aggressive impulses).

Analysis of protocols in piecemeal fashion by individual stories inevitably involves the loss of some interpretative data. For this reason a *sequence analysis* of entire protocols was undertaken. Each protocol was analysed for alternating identifications throughout the series of six stories; and it was then subjectively decided whether these alternating identifications represented a conflict of impulses (e.g. aggressive identifications alternating with passive-dependent identifications were considered to represent a conflict of these impulses). Each protocol was classified only

Table 5

Main conflict	No. of delinquents	No. of non-delinquents
Dependence versus aggressive antisocial impulses	18	6
Aggressive antisocial impulses versus fear of consequences	7	2
Miscellaneous conflicts	4	3
No conflict of identifications	2	25
Total	31	36

SUMMARY OF METHOD OF T.A.T. ANALYSIS

(1) Distinguish identificant (i.e. the character to whom feelings are attributed, or whose needs are most elaborated, or from whose point of view the story is written).

once according to what seemed to be the dominant conflict. The results are given in Table 5.

Although this analysis is merely subjective, the suggested trends are of interest. First, it is noted that only two delinquents as against

25 non-delinquents are classified as showing no conflicting identifications. Some measure of conflict in non-delinquents can, however, be assumed from the main analysis, which seemed to suggest a conflict of aggressive, antisocial impulses versus guilt. Secondly, 18 delinquents, against six non-delinquents, are classified as having a conflict of aggressive and passive-dependent needs.

DISCUSSION

The findings emphasize the importance of superego development as a check against the aggressive, antisocial impulses which seem to be common to both groups; and the oft-quoted hypothesis that delinquency is guilt-motivated because of a need for punishment is not supported. On the contrary, it is the defences of the non-delinquents which are guilt-motivated. The delinquents lack these defences, and show little evidence of guilty or intro-punitive reactions.

The concept of *psychological distance* (Tomkins, 1947) between the self-percept and the antisocial impulses can be used to explain many of the differences between delinquent and non-delinquent T.A.T. protocols. It was seen, in the 'classification of themes' experiment, that there was little difference between the two groups in regard to the number of aggressive and antisocial stories.

The difference was in the presence or absence of adequate defence mechanisms, which were interposed between the guilty impulse and the (phantasied) act. In the non-delinquent group this distance was effected either by *denial* of the reality of these impulses (It isn't me, it's someone else'; 'It isn't true, it's only a story'); by *inhibition* due to guilt feelings; ('my conscience would prevent such an act, or punish me severely for it'); or by *rationalization* ('I could never voluntarily do such a thing, but only if I were not responsible for my own behaviour, or if morally justified'). In Tomkins's (1947) terminology, the delinquents show a pathological lack of distance between the aggressive antisocial phantasies

and their expression. The other non-delinquent criteria, 'social and moral values' and 'social achievement' are evidence of sublimatory mechanisms arising from adequate superego development.

As regards T.A.T. analysis, it seems important to interpret the behaviour of the identifi-cants and to classify this according to theoretical knowledge of the various syndromes. Discovering the identifi-cant and analysing the defence mechanisms implicit in the themes is an essential but not a simple task. The procedure described requires experience, but used in this fashion, the T.A.T. can be made to differentiate meaningfully between delinquent and non-delinquent groups. It is suggested that a mere classification of themes will be of little value.

No claim is made for this system as a method of predicting recidivism. It may well be that other T.A.T. cards will differentiate better than those chosen; and replication of the experiment is necessary before the recommended cut-off point can be used with confidence.

SUMMARY

On the basis of several *a priori* hypotheses derived from experience with juvenile delinquents, two psychologists were each able to differentiate delinquent from non-delinquent T.A.T. protocols significantly better than chance. Since a mere classification of themes had failed to do this, it was concluded that, to be of use, a T.A.T. must be *interpreted*. It is not the theme, but the manner of its organization, which can reveal personality characteristics.

The importance of discovering the *identifi-cant* in each story became apparent, as did the need to classify the behaviour of the identifi-cant according to theoretical knowledge of the various syndromes. The importance of analysing the defences implicit in the stories was discussed and these defences were considered in terms of the 'psychological distance' which the testee puts between the forbidden impulse and the (phantasied) act.

It was concluded that T.A.T. analysis required clinical experience in T.A.T. interpretation. Used in the way described, the

T.A.T. can be made to differentiate between delinquent and non-delinquent groups of male adolescents in a meaningful fashion.

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REVIEWS

Battle for the Mind: A Physiology of Conversion and Brain-Washing. By WILLIAM SARGANT. (Pp. 248. 25s. net). London: Heinemann. 1957.

In 1944, Dr Sargant discovered Pavlov. After reading *Conditioned Reflexes and Psychiatry*, he was quick to see striking analogies between the phenomena of 'experimental neurosis' as described by Pavlov in dogs and the clinical manifestations of battle stress and drug abreaction with which he was predominantly concerned at the time. Not long afterwards, he chanced on a copy of John Wesley's *Journal*. Here he discovered a vivid account of the emotional states which might be brought about by the particular kind of preaching upon which Wesley built his reputation. The parallel between abreaction in cases of combat neurosis and these extremes of Revivalist fervour was irresistible: 'The two techniques were startlingly similar'. Thus this book was born.

Dr Sargant opens with a brief exposé of Pavlov's work on conditioned reflexes. This is clear but inevitably condensed. He then attempts a comparison between the manifestations of combat fatigue and battle stress in man and certain aspects of animal behaviour which have been studied in conditioned reflex experiments. This is followed by a brief account of drug therapy, psycho-analysis, shock treatment and leucotomy. Thus the stage is set for the most important part of the book, viz. the elucidation of religious conversion, brain-washing and the eliciting of confessions, on the basis of neo-Pavlovian theory. These various topics are dealt with in successive chapters of the highest interest and value. (Mr Robert Graves contributes a chapter all his own on 'brain-washing in ancient times'.) There is a penultimate chapter on 'consolidation and prevention' and a final summary of conclusions. Mention should also be made of the many dramatic photographs which embellish the text. There is a short bibliography and a serviceable index.

The broad—and bold—thesis of this book is that experimental neurosis, combat fatigue, abreactive arousal and collapse, and certain as-

pects of indoctrination and conversion, may all be comprehended within the framework of Pavlovian theory. For example, combat fatigue is viewed as the outcome of 'transmarginal inhibition' and is often found to follow upon a state of general hyperactivity, identified by Dr Sargant with the excitatory phase of experimental neurosis. Drug abreaction is likewise explained in terms of cortical dis-inhibition; its subsequent 'collapse phase' being again related to 'transmarginal inhibition'. Pavlov's crude distinctions between 'temperamental types' in dogs are found to be broadly applicable to human psychiatry. Conversion and brain-washing are regarded as essentially phenomena of reconditioning, facilitated by severe pre-existing physical or emotional stress. Psychotherapy itself is viewed as a form of indoctrination open to explanation in terms of Pavlovian principles. 'By an assemblance of relatively simple mechanisms and physiological studies', Dr Sargant concludes, 'it has been shown here that not only certain methods of religious and political conversion—whether practised on groups or individuals—but also some of the results of psychoanalysis, of drug abreaction and of shock therapies...may start to be better understood in relation to one another.'

Although the above account is certainly too summary, and does less than justice to Dr Sargant's insight and modesty, it is perhaps sufficient to indicate the nature and tone of his argument. In the present writer's view, his book raises issues of the utmost importance not only for psychological medicine but for our whole conception of human behaviour. If only for this reason, it would seem essential that Dr Sargant's thesis should be submitted to the most careful scrutiny. The following comments are intended to serve no purpose other than to stimulate discussion of this important and fascinating book.

(1) *Pavlovian theory.* No one will deny that Dr Sargant has performed a most useful service in directing attention to Pavlov's important work on animals and its possible relationship to behaviour disorders in man. Undoubtedly the phenomena are in some cases homologous and in others strongly suggestive of similarity in under-

lying mechanism. Yet he appears to have accepted Pavlovian theory *en bloc* with insufficient regard for its very real difficulties. Few neurophysiologists brought up in the post-Sherringtonian climate have found it possible to take Pavlov's theories—as opposed to his facts—seriously. They just do not cohere with the body of Western neurophysiological knowledge. Moreover, a leading neuroanatomist recently referred to Pavlov's theories as so unsatisfactory in the light of what is reliably known about the structure of the cerebral cortex as scarcely to warrant serious consideration. This does not of course mean that Pavlovian theory is necessarily wrong or lacking in heuristic value. It does, however, mean that it is esoteric, controversial, and hence perhaps an insecure foundation for Dr Sargant's psychological superstructure.

(2) *Combat fatigue and battle stress.* These states are interpreted by Dr Sargant along classical Pavlovian lines, with the emphasis upon excitation, inhibition, and the various types of interaction between these two supposedly fundamental aspects of cortical activity. Yet the Pavlovian approach, one may point out, has completely ignored the homeostatic and other mechanisms concerned in bodily regulations at levels lower than that of the cerebral cortex. Further, it has long been clear to psychologists that Pavlovian theory, in its classical form at least, provides no really satisfactory way of handling problems of motivation. Even in the case of experimental neurosis in animals, many workers have felt constrained to broaden the structure of Pavlovian explanatory theory in order to comprehend its motivational aspects. Dr Sargant gives no indication of these limitations of Pavlovian theory and seems on the whole content with rather facile analogies between the clinical and experimental data.

(3) *Conversion and brain-washing.* Fundamental changes in attitude and belief, whether gradual or sudden, Dr Sargant treats as essentially new conditioned reflex patterns. He places great emphasis on physical and emotional exhaustion—with all the changes in cerebral efficiency pre- with all the changes in cerebral efficiency pre- sumably attendant upon it—as an essential condition of these changes. (Indeed here the link between re-education following drug abreaction and brain-washing is most clearly perceptible). Now it seems impossible to deny that states of extreme physical or psychological stress play a most important part in conversion experiences

and brain-washing procedures. These states may clearly be expected to impair or otherwise modify cerebral function in such a way as to facilitate drastic changes in attitude or belief. Yet to ascribe the resultant changes (whether spontaneous or the outcome of indoctrination) wholly to conditioning appears as naïve as Pavlov's own incautious statement that 'the different kinds of habits based on training, education and discipline of any sort are nothing but a long chain of conditioned reflexes'. If J. B. Watson's Behaviourism has taught us nothing else, it has at least convinced us that attempts to base all human learning on the conditioned reflex formula just won't do.

(4) *Psychotherapy and Indoctrination.* Dr Sargant appears to have no doubt that psychotherapeutic procedures are basically processes of indoctrination. He makes no distinction between techniques based explicitly on persuasion or suggestion and those entailing some measure at least of rational understanding. This might seem short-sighted. Whereas it is entirely possible that psychoanalysis involves more in the way of indoctrination than is commonly supposed, at bottom its intention is to liberate judgement and to permit some small increase in the rational control of behaviour. This it may do either by giving the patient access to facts about himself of which he was formerly, in part at least, unaware, or by weakening emotional resistance to rational inference and decision. Whether in fact it succeeds in doing so (and even Dr Sargant seems inclined to admit that it may do so in certain instances) is basically irrelevant. The point is that just as conditioned reflex theory has failed to explain 'learning by understanding' in the laboratory or the school, so does it fail to explain 'improvement by insight' in the clinic.

In view of these considerations, it might seem a pity that Dr Sargant has made no attempt to place his clinical findings within a broader theoretical framework. Some years ago, Mira made a tentative attempt to interpret cases of battle stress very similar to those studied by Dr Sargant in terms of the Jacksonian doctrine of dissolution of function in the central nervous system. Like Dr Sargant, Mira regarded fear and rage as essentially inhibitory states, though he did not attempt to relate them to any particular theory or cortical inhibition. His idea was that progressive emotional stress leads to progressive dissolution of higher-level function, with concomitant release of

earlier, simpler and more highly organized patterns of reaction. This approach not only makes possible more adequate recognition of the psychological facts of emotion but accords far more closely with traditional neurological thinking. Though vaguer, perhaps, than the theory of Pavlov, it at least lacks spurious precision.

It is also difficult to avoid the inference that Dr Sargant regards a theory expressed in physiological language as intrinsically more plausible than one expressed in psychological language. Thus 'transmarginal inhibition' appears to have far higher 'reality value' for him than (say) 'repression'. He comments that the similarities between human and animal neurosis would be difficult to comprehend '... unless it be conceded that Pavlov's dogs had subconscious minds, and also super-egos, egos and ids'. He does not seem to realize that Pavlovian inhibition is just as mythical as the Freudian unconscious—both are but concepts serving in their appropriate frames of theoretical explanation. The question is surely which theoretical frame is most useful in relation to the phenomena one is trying to explain. Even in the case of dogs, the appropriateness of the Pavlovian scheme to cover *all* the facts of experimental neurosis has been questioned by several workers, Liddell and Masserman among them. In the case of human neurosis, its appropriateness might seem even more debatable. This does not mean that the Freudian (or indeed any other) scheme is necessarily to be preferred. It does, however, mean that theories should be assessed on their merits rather than in terms of an obscure metaphysical bias.

Lastly, it will surely seem to many people that physical fatigue and emotional stress, with the alterations in cerebral functions which they presumably entail, do no more than provide a background favourable to drastic changes in psychological outlook. For instance, the testimony of those who have been exposed to brain-washing strongly suggests that social disconnexion is far more important than physical isolation, and that the creation of uncertainties regarding existing motives, values and beliefs is far more important than the impairment of judgement directly resulting from stress. Ideological conversion, indeed, seems to be very much the outcome of gradual awareness of the irrationality and insufficiency of a system of beliefs and values hitherto accepted without question. Although the new system may be no

more rational than the old, it is accepted because it does at that particular moment appear to the subject to square better with what he now conceives to be the true facts about the human situation. It is surely in the creation of perplexity, in the challenge to all accepted values and ideals, that brain-washing resembles psychoanalysis. But in the one case the aim is to induce the subject to accept an arbitrarily imposed set of values in the interests of political expediency; in the other to choose his values for himself, given better understanding of the nature of his own motives and psychological history. For all its doctrinaire aspects (and Dr Sargant is entirely right in supposing that they exist), psychoanalysis stands firmly within the Renaissance tradition of respect for reason, for liberty and for the individual.

The present reviewer would be the last person to hold that psychology should not be viewed as a biological science. With Lashley, he looks forward to the coalescence of psychology and neurology in an integrated science of behaviour. Dr Sargant on the other hand, appears at bottom a dualist whose conception of psychological medicine still remains *mens sana in corpore sano*. The task of medicine, he seems to say, is limited to body and brain: given optimal cerebral health psychological health will look after itself. Up to a point he is probably right. Yet at the present stage of knowledge it is difficult to see how psychological medicine can profitably pursue its business wholly on the basis of physiological theory. One might hazard the view that what is needed is a more broadly conceived neurology—a neurology in which the central nervous system is regarded not as a reflex machine but as the instrument of real human behaviour. Perhaps Pavlov has indicated the beginnings of such a neurology. But he has not—as a lay reader of Dr Sargant's book might be forgiven for imagining—already achieved its creation.

O. L. ZANGWILL

The Mentality of Apes. By WOLFGANG KÖHLER. (Pp. 286. 3s. 6d.) Translated from the 2nd revised edition by ELLA WINTER. Penguin Books Ltd. 1957.

It is pleasant to welcome this classic of modern psychology in its newly fledged Pelican guise.

Besides providing in its day a timely counterblast to Behaviourism, Prof. Köhler's well-known book may be said to have laid the foundations of what has now come to be called 'field theory'. Although the latter is known principally through the work of Lewin, the basic idea of the 'psychological field'—with its complex structure of vectors, valencies and barriers—first became explicit in Köhler's brilliant analysis of problem-solving in chimpanzees. His concept of insight, too, can still claim value as an indispensable (if indefinable) tool in psychological analysis. Although the Gestalt use of the term differs in important ways from that of the psychopathologist, it seems not improbable that the basic idea is at bottom very much the same. In man no less than in the ape, constructive solution of new problems implies a capacity to detach objects from their familiar settings and to see old relationships in new ways. Although the obstacles to insight in man are internal rather than external, its basic characteristics at the different phylogenetic levels may well turn out to be less dissimilar than is often supposed. At all events, no one interested in the comparative study of behaviour can afford to ignore this remarkable book.

One may perhaps regret that Prof. Köhler has not seen fit to add a note about more recent experimental work on primates and its implications for his theories. It has become clear, for instance, that 'insight' has a history: without previous familiarity with sticks, boxes, etc., insightful tool-using by apes seldom, if ever, comes about. Again, the differences between 'insight' and 'trial-and-error' types of learning have turned out to be altogether less hard and fast than was at one time supposed. Further, the important relations between insight, perception and instinct recently adduced by Lorenz, Thorpe and others raise exciting theoretical possibilities. None the less, this book still stands securely on its own feet and its interest to-day is far from being merely historical. It remains as one of the very few books in psychology really worth reading.

Ella Winter's translation is competent and clear. Unfortunately, the photographs that added so much to the attractiveness of the 1925 edition have been left out of this one. No doubt there were telling reasons for their omission but it is none the less a pity. The frontispiece, at least, might have been retained.

O. L. ZANGWILL

The Psychology of Perception. By D. W. HAMLYN (ed. R. F. HOLLAND). (Pp. 120. 12s. 6d.) London: Routledge and Kegan Paul. 1957.

Mental Acts. By P. GEACH, (ed. R. F. HOLLAND). (Pp. 136. 12s. 6d.) London: Routledge and Kegan Paul. 1957.

These two books are the first volumes to appear in a series of projected monographs under the general editorship of R. F. Holland. As the editor makes clear in his Preface, they are not perhaps so much monographs as *discussions*; they do not so much present the current evidence with detailed contextual references, as present a philosophical point of view. They are both philosophical essays, concerned with conceptual rather than empirical questions or issues, though experimental and other evidence is important to both. Facts are important here in two ways: the theories which are examined, such as the Gestalt theory of perception are, at least *prima facie*, concerned to describe and explain facts, and need only be taken seriously if they appear to have some success in this, and so facts are clearly important in evaluating the importance and truth of the theories to be discussed. But also, the philosophical discussions presented are of the type aimed to help us to describe the facts we discover in ways which are not misleading. Both books are primarily concerned with the language we use to describe facts about the world, or about perception, or about mental acts.

It is now very clear, primarily from the work of Wittgenstein, G. E. Moore and A. J. Ayer, that when we try to talk about some problems the structure of our language may mislead us, perhaps by suggesting analogies which are inappropriate. This should not really be surprising for language must have developed primarily for communication about the mundane things; about the growth of crops, and how to build houses, and whether it is raining; to ask questions about health, about debts or great victories, or intentions or hopes. But when we ask such questions as 'How do we see?', or—when we look at a table—'Do I really see the table *itself*?', then our language used for the answer is the tool developed to deal with house building, and farming, and accountancy and so on. Words such as 'object', although all we have them, become inappropriate. We are normally concerned with physical objects, and our reactions to them; when we ask: 'What

is a physical object?' we are not trying to specify which, or what sort of object it is, but rather are we attempting something never attempted at all by non-philosophic man, though we use his language and analogies. This is old stuff now, of course, but it always seems worth repeating in slightly varied form.

Both these books point out subtle pitfalls where language misleads at least some people some of the time, and in this they perform a valuable service. Of course the reader is always apt to think: 'Yes, of course, but I am not misled in this silly way'. Yet, looking back on a dose of such reading, one can generally detect a change in attitude, a clearing of the smoke after the battle of the words. But, like other battles, the result may not be clear, and an apparent victory may bring new troubles.

Hamlyn discusses the nature of explanation in the psychology of perception, Gestalt theory, illusions and their significance, and the role of needs and motives: the so-called 'New Look' in perception. He does not discuss in any detail the experimental findings of workers in the field, or the neurological basis, so far as it is understood. He does, however, discuss the *sort* of things we should say when talking about perceiving, and so he is making recommendations for the use of language. Now are his recommendations useful? Does he point to cases where the language of the man in the street, or neurologists, or other philosophers or experimental psychologists have really caused wrong deductions, or experimental errors, or mystery where there could given the same facts have been a clear light of understanding? Probably he does succeed in some cases.

A main point, stressed throughout the book, is that neurology cannot in principle give us a theory of perception because, so he argues, neurological theories can give only some of the necessary conditions for perception and not the sufficient conditions. In the opinion of the reviewer, Hamlyn is in danger of creating his own confusion here: it is difficult to think what, ultimately, an explanation of perception could be but a story about the working of the nervous system under various 'input' conditions. What else can we ask for? If we knew this answer we should understand perception as we understand wireless sets, or cars, or any functional system which we can make, or whose manner of working we can describe. If this is indeed the only sort of

appropriate explanation then how can Hamlyn be right here? When Hamlyn objects to neurological theory as being inappropriate for understanding perception, it appears that he uses such words as 'perception' and 'behaviour'—he would also object to neurological explanation for behaviour, such as walking—as including in their meaning more than the 'output' activity of the organism. He would include norms, achievement and so on, before he would say that, for example, putting one foot in front of the other is walking. Thus he might accept neurological description as sufficient to explain *activity*, but not *behaviour*. Now this distinction between 'activity' and 'behaviour' did not appear clear in the book, and I am indebted to him for private elucidation. Perhaps if it had been made clear the argument might have appeared somewhat trivial, though still worth pointing out. As it is the discussion is confusing.

Geach is concerned largely with Bertrand Russell's theory of meaning, described most fully in that difficult book: *An Inquiry into Meaning and Truth*, which appeared in 1940. Geach's book is scholarly, and has many valuable criticisms and original points. It is highly specialized, and though likely to prove an important contribution is perhaps an odd choice, at least for an early volume, in a series presumably devoted to the psychologist rather than to the professional philosopher.

R. L. GREGORY

Emotional Problems of Early Childhood. Ed. G. CAPLAN. (Pp. 544. 42s.) London: Tavistock Publications.

In 1954, the International Association for Child Psychiatry held in Toronto an unusual type of congress, in the form of a working institute. Fifty-seven contributions were submitted from a number of countries, and a substantial number of these were selected for presentation by an international Preparatory Commission, and by a miracle of organization were presented during the two short days of the institute at Toronto. It was then decided, with the same originality, to use this material as the basis of a book, in place of the usual proceedings. This involved a further selection process, and the exclusion of a number of valuable papers; but the result of this distillation is a symposium of exceptionally high quality and unusual readability.

The volume consists of twelve case studies and nine research reports, grouped in three sections: (1) preventive aspects of child psychiatry, (2) the relation of physical and emotional factors, and problems of hospitalization (originally two themes, merged for reasons of economy) (3) problems of psychosis in early childhood. Each case study is followed by a discussion in which the points raised in the Toronto meetings are skillfully woven by the editor into a unified and balanced whole. A concluding chapter reviews the basic theme of each section and fits the various contributions into their place in the overall picture of current trends and existing knowledge. These devices give the book a much more satisfying degree of unity than is usual in a symposium.

Comparatively little had been previously published on the important subject of preventive child psychiatry, so this section is of particular interest and value. Much of the early work in this field was done in the Wellesley Human Relations Service, Boston, and in the Lasker Centre for Mental Hygiene in Jerusalem. Each of these centres is represented by a case study and by a research report concerned with methods of identifying those children whose mental health is likely to need protection, the one by the development of a predictive test of social adaptation, the other by the systematic study and evaluation of the mother-child relationship. Dr Gerald Caplan, who has connexions with both centres, contributes a masterly final chapter.

The second section contains American case studies of ulcerative colitis in a young child and severe emotional disturbances in a child with a cardiac disability. In both cases a gross distortion of the mother-child relationship seems to be a central pathogenic factor. British research on maternal deprivation (which is well documented elsewhere) is not represented, but a group of French workers contributes an account of a research programme concerning deprived children under three, and a case study of a severely disturbed deprived child and her treatment. There are American studies of hospitalization and are psychosomatic disorders in children, and a Canadian one on suspected deafness in pre-school children. (30% proved to be pseudo-deaf, and just under half of these to be autistic.) In the final chapter, Dr Fred Allen reviews the subject of mother-child separation from a Rankian stand-point, stressing that psychological separa-

tion is an essential component of normal development.

The very valuable third section presents six case-studies and two research reports on infantile psychoses, on which material has hitherto been available only in scattered articles. Both out-patient and in-patient treatment is described, and methods from electric convulsions (Bellevue, New York) to the modified psychoanalytic technique of the Putnam Centre (Boston) the Rankian technique of Knowlton and Burg (Philadelphia) and the 'vegeto-therapy' of Nic Waal (Oslo). Three cases showed an encouraging response to psychotherapy, though the durability of the improvement obtained is still unproved. The research reports represent the theoretical extremes. Dr Beata Rank (Putnam Centre) prefers the term 'atypical child' and outlines a research project whose main hypothesis is that this type of child 'has suffered gross emotional deprivation and in self-defence has isolated himself from the unsatisfactory emotional environment.' Dr Lauretta Bender (Bellevue) usefully summarizes twenty years of research on schizophrenic children, which has led her to define schizophrenia as 'a maturational lag at the embryonic level in all the areas which integrate biological and psychological behaviour; an embryonic primitivity or plasticity characterizes the pattern of the behaviour disturbance in all areas of personality functioning. It is determined before birth and hereditary factors appear to be important. It may be precipitated by a physiological crisis, which may be birth itself....' Clearly, there is something here for everybody

ELIZABETH E. IRVINE

The Psycho-analytic Study of the Child. Vol. XI. (Pp. 470. \$8.50). New York: International Universities Press. 1956.

Published in the Freud Centenary year, this is an especially rewarding volume of the Psycho-analytic Study of the Child. We have come to expect from this Annual not only rich clinical material from child analysis, but also some of the most outstanding theoretical papers to appear in post-war psychoanalytic literature. If anything, vol. XI surpasses its distinguished predecessors in the quality of both types of contribution.

The theme which unites the theoretical papers is the relation between phantasy and reality, and

they include what may come to be judged the most brilliant of the late Ernst Kris's theoretical papers: 'The Recovery of Childhood Memories in Psycho-analysis'. In this paper, Kris dealt with the structural, dynamic and technical aspects of the recovery of memories during treatment, and in so doing, he re-opened the problem of the paradoxical role of memories which Freud had raised when he wrote to Fliess in 1897 on the 'Architecture of Hysteria': 'phantasies are psychical outworks constructed in order to bar the way to . . . memories (of primal scenes). At the same time, phantasies serve the purpose of refining the memories, of sublimating them.'

Hartmann's 'Notes on the Reality Principle' is a major contribution to his series of papers which clarify and extend Freud's structural theory, and Phyllis Greenacre in 'Experiences of Awe in Childhood' discusses the 'lighting up' of ego functions by sexual cathexes. Psychologists who have been paddling rather timidly in the shallows of 'perceptual defence' and 'subception' problems will find that these papers, together with Lustman's 'Rudiments of the Ego' have much to offer them on the relation between perception, thought and affect.

Special mention should be made of Elizabeth Zetzel's paper on the relation between concept and content in psychoanalytic theory. This is the most constructively thoughtful and subtle account yet to appear of the differences between Melanie Klein's approach and that of present-day Freudians. Zetzel is peculiarly well qualified for her task in this paper as her earlier work was with the British School of psycho-analysis but as a member of the Boston Psycho-Analytic Society, she has since become well acquainted with the broad streams of current psychoanalysis.

In the section on 'Normal and Pathological Development', Alpert and her colleagues report some interesting studies of babies who showed unusually strong or weak or unbalanced oral and activity drives; Beres demonstrates by clinical examples the role of the ego in schizophrenia; and Kestenberg discusses the development of maternal feelings in both young boys and girls. Her paper is a most valuable contribution both to clinical psychoanalysis and to the general understanding of child development.

Of the many interesting clinical contributions, two may be indicated as of special interest: Erna Furman's case-study of a severely disturbed three-year-old girl, and Joyce Robertson's sensitive and

revealing account of her four-year-old daughter's reactions to a tonsillectomy.

A 'must' for psychoanalytic workers in both child and adult practice, this volume will also give the non-analytic medical or clinical psychologist an excellent indication of what is going on in some of the most advanced psychoanalytic thinking of to-day.

CECILY DE MONCHAUX

Studies in Hysteria. By JOSEF BREUER and SIGMUND FREUD, Translated and edited by JAMES and ALIX STRACHEY. (Pp. 335. 25s.) London: Hogarth Press and Institute of Psycho-Analysis. The International Psycho-Analytical Library no. 50. 1956.

This is a reprint of the excellent new translation of 1955, previously available as vol. II of the Standard Edition of Freud's Work, and now published separately. It contains a lucid and comprehensive editor's introduction describing the history of the work, its importance for psychoanalysis and the divergencies between the two authors.

The book itself reveals to us the gradual emergence of psychoanalytic theory and technique from hypnotic therapy.

It begins with the 'Preliminary Communication' previously published in 1893, two years before the 'Studies'. There follows Breuer's famous case history, Frau Anna O, and four cases by Freud. With Freud's first case we are confronted by the virility of his uncompromising style and realize that Breuer's account has been half-apologetic. Nevertheless, the case makes difficult reading, probably because Freud attempts a more complete explanation than was possible without the tools he later developed. In the later cases his accounts give growing conviction.

Breuer's theoretical chapter is the product of a brilliant mind, but one not able to rise above current scientific thought quite as Freud's could.

In the final section one sees Freud free himself from his own resistances and from the uncertainties of his friend Breuer and come right out with a masterly description of the defence mechanism.

This book is more than a scientific document of historical value. It is a living account of Freud's greatness. It is perhaps surprising that Freud himself should have later valued it so little.

PETER LOMAS

several good stories, some of recent vintage, and he can write wittily himself. He is often provocative, but never dull. Dealing with topics as diverse as the circus and the clown, the strip tease, the mystery story, Homer, the Bible, and laughter in the psychoanalytic situation, he usually has something clever or wise to say. With Freud as his guide he really has got to the heart of the matter and has confirmed and broadened our insight.

The book is beautifully produced and has a number of apt and amusing illustrations. The price is high by British standards, and this may prevent it from reaching as wide an audience as it deserves.

VICTOR KANTER

Sense and Nonsense in Psychology. By H. J. EYSENCK. (Pp. 349. 3s. 6d.) Harmondsworth: Penguin Books Ltd. 1957.

In his introduction, Professor Eysenck describes the present book as 'in a sense... a sequel to *Uses and Abuses of Psychology*'. Compared with the earlier book it is more specialized in content—although the author states that he has 'taken a wider field'. The eight topics chosen are grouped into two parts, entitled 'borderlands of knowledge' and 'personality and social life.' The first covers hypnosis and suggestibility, lie detectors and truth drugs, telepathy and clairvoyance, and the interpretation of dreams. The second contains chapters on the 'measurement' of personality, personality and conditioning, politics and personality, and the psychology of aesthetics.

Most of the chapters contain much to interest the non-technical reader, to whom the book is of course addressed. That being so, it is perhaps unreasonable to expect detailed references to research findings which the author quotes in support of his own view of a controversial issue. In consequence the uninformed reader may, perhaps, too readily assume that certain issues have indeed been settled. This does not refer to several of Prof. Eysenck's main topics: thus, he gives a lively but dispassionate account of the evidence for the reality of hypnotic phenomena and of the statistical support for the occurrence of some sort of extrasensory cognition, about the nature of which he wisely refrains from speculating. It is in relation to more fundamental psychological issues, which have formed the subject of his earlier,

polemical writings, but which are here marginal, at best, to his ostensible themes, that Prof. Eysenck allows emotion to colour his presentation. These topics may conveniently be grouped together to form a single theme which Eysenck is at great pains to drive home at any point in his writing, however irrelevant, namely the general lack of respectability of psychoanalysis and psychotherapy and any of their applications or ancillary studies, such as projective techniques. Thus the concluding chapter, on the psychology of aesthetics, which consists of a comprehensive account of experimental work on preference judgements—but which commits the cardinal error of equating aesthetic experience with sensuous pleasure—ends with a totally irrelevant digression ridiculing psychoanalytic interpretation in the study of literature and biography. Again, at the end of the chapter on hypnosis and suggestibility, Clark Hull having been quoted to the effect that 'what is lacking in most of the early work on hypnosis is the notion of a controlled experiment', two pages follow in which 'the problem of the effectiveness of psychotherapy' is taken 'as an example of the need for controlled experiments'. Reference is made to 'a proper experiment...' (reviewer's italics) 'in which matched groups of neurotic subjects were respectively treated by psychotherapy and not treated at all'. The author has the grace to admit that such an experiment may involve ethical problems; what his own answer is need not detain us, but it is to be hoped that psychologists, as well as physicians, are to be found who will say, with Albert Camus: '*On ne peut pas en même temps guérir et savoir. Alors guérissons le plus vite possible. C'est le plus pressé.*'

In his foreword, the general editor of the series remarks that 'interesting and potentially explosive issues should be matters of rational discussion'. That Prof. Eysenck is capable of this when it suits him is abundantly evident, and the greater is the pity that so much of his polemics should consist of tasteless gibes which it would be equally tasteless to quote verbatim in a short review. It is perhaps still more unfortunate that even the more universally acceptable parts of the book should be marred by evidence of having been hastily put together. It is not to work of this sort that one must look for furtherance of the public relations of psychology, at any level.

BORIS SEMEONOFF

The Sentence Completion Method. By AMANDA R. RHODE (Pp. xii+301. £3.) New York: Ronald Press Company. 1957.

This book is a manual for the Rhode Sentence Completion Method, which is a pencil-and-paper projective test taking about an hour to give, but easily administered either individually or in a group setting. The patient is given the first few words of 65 sentences which he is required to complete in writing, the items being designed to elicit material about the patient's feelings, goals, cathexes, attitude to family, etc. The data may be scored quantitatively and reasonably objectively, or in skilled hands may be used to provide a qualitative assessment of personality.

Two dozen or so case studies are included demonstrating both approaches to interpretation, but the greater part of the book is given to chapters on standardization on two normal groups (adolescents and adult males) and six clinical groups (dysthymics, a mixed psychoneurotic group and a schizophrenic group subdivided into the four classical types). The author advocates the use of Murray's scoring system of needs and press, and this method is fully described.

The normative data includes mean scores and standard deviations of each group for each variable in Murray's classification, but the size of the group is rather small in most instances and it would seem that norms of this kind are unlikely to be of great value to the clinician in the assessment of individual records. However, the author's tables bristle with interesting problems for further research.

This is a useful book, being a clear and scholarly manual for a test which has so far been little exploited in this country but which would seem to have considerable clinical and research possibilities.

J. L. BOREHAM

The Analysis of Fantasy: The Thematic Apperception Technique in the Study of Personality. By WILLIAM E. HENRY. (Pp. 305. 48s.) London: Chapman & Hall Ltd. 1956.

Dr Henry's contributions to the Thematic Apperception Technique have had a considerable in-

fluence on its development during the past ten years. The present volume will undoubtedly extend this influence because he has well succeeded in his aim to 'formulate practices and principles in ways sufficiently clear to permit systematic investigation and a transmittal of these principles to colleagues and students'.

His early chapters on the social and psychological nature of the task of interpretation, the principles of the interpretation process, and the basic nature of the test materials, make a most valuable contribution to the rationale of the T.A.T. The dynamic concepts used are clearly stated and developed, yet with sufficient generality to be acceptable to and usable by students and workers with widely different theoretical preferences.

These chapters are followed by a careful examination of the variables of form and of content in T.A.T. stories, which leads on to the presentation of a conceptual framework for individual case analysis, within which the data may be systematically ordered and interpreted.

In Part II of the book detailed analyses of T.A.T. stories from adolescent and adult subjects are presented. In Part III there is a most valuable summary of normative information about Murray's T.A.T. pictures, some useful comments on the administration of the test, and the most complete bibliography on T.A.T. so far published.

Throughout the book Dr Henry's concern is with a non-clinical formulation of personality, yet his exposition of principles and practice with this projective technique are especially valuable for students of clinical psychology as well as for those concerned with social and personality studies more generally. The method and approach which Dr Henry illustrates so well require a systematic interpretation of the data, with constant references to the stories. Such a discipline provides an essential background of training for all students wishing to use T.A.T. or any other projective technique.

This book has much to offer to the teacher in universities and post-graduate courses in psychology, as well as to students, research workers and to clinical psychologists who are concerned with the development and use of projective methods in social and individual psychology.

H. PHILLIPSON





ERNEST JONES

(Frontispiece)

ERNEST JONES*

1 JANUARY 1879 TO 11 FEBRUARY 1958

BY EDWARD GLOVER

In our motive-hunting and consequently somewhat disillusioned age it is often said of obituary addresses that they steer a precarious course between the polarities of human ambivalence. One of these polarities is still most compactly expressed in the Latin tag *De mortuis nil nisi bonum*; or, to borrow from Ernest Jones's own quiet paraphrase, a constraint 'to embellish what might otherwise be a more sober judgement'. The other, if more masked, constraint is to express in terms of apparently sober judgement some of the animus that has been provoked during the lifetime of the subject.

All this is no doubt true enough. But I venture to think that it misses the immediate function of post-mortem eulogy. For when the envies that men of distinction have aroused during their lifetime are, in the words of Francis Bacon, extinguished by their death, the way is open to pay tribute to greatness. The way is open to pay tribute to greatness. 'The temple of fame', said Hazlitt, 'stands upon the grave.' In this sense the true function of eulogy is cathartic; it enables the living to express through the medium of praise those allegiances to greatness which, in the run of their daily life, are so often muted. Let us therefore not cavil over the priorities of distinction or pre-eminence, maintaining only the simple standard that a man may be judged by his works, whether these lie in his designed cultural activities or within the more restricted confines of his private character.

This standard is one that Ernest Jones reached and surpassed with an apparent effortlessness that gave little clue to the intense and disciplined energy he poured into his

dedicated career. For no one who came in intimate contact with him could fail to recognize the twin elements of his eminence, namely, the complete dedication of his life to a cause and the manner in which he was matched with the psychological age in which he lived. Critics of psychoanalysis have not been loath to maintain that it is one part a science, one part an art and one part a substitute for religion. The description is summary but not entirely without truth. Ernest Jones was most assuredly a scientist, and in his own lifetime was, though tardily, admitted to that freemasonry; as a technician in the handling of human conflict he was an artist; but behind and beyond all this he was intensely aware of the cultural mission of psychoanalysis, viz. to bring the spontaneous adaptations of man more fully within the ambits of reality and reason. 'Where Id was', said Freud, 'there shall Ego be', an almost heraldic device that indicates the humanistic cause by which Jones was fired and which guided him throughout his professional life.

At this point one can readily imagine Jones shaking his head in vigorous dissent. Such incentives, he would have maintained, are part and parcel of a scientific *Weltanschauung* and call for no religious motivation. Yet I doubt whether it is possible for anyone to concern himself about the future behaviour of mankind without drawing on the fortifying energies of a humanistic faith.

Yet another source of his dedication is not hard to find. His allegiance to psychoanalysis was nourished throughout by a personal allegiance to its founder Freud. To be sure Jones was no idolater; his realism would not permit him to indulge such filial over-idealizations. Indeed, amongst that original band of brothers, the Freudian Advance Guard or 'Committee' as it was called, which Jones founded and of

* Obituary address by Dr Edward Glover to the Medical Section of the British Psychological Society, 26 March 1958.

which he was the last surviving member, he stood pre-eminent for sanity and keenness of judgement. Nevertheless, the role he played as champion of psychoanalysis throughout the English-speaking world was clearly sustained by an Ego-ideal which was remarkable for its similarity to that of Freud himself. And it is not without significance that there was also a striking similarity in the progress of Freud and Jones from natural science to psychology. It was through this coalescence of identifications (or as some might say introjections), of Ego-ideal and real Ego that Jones expressed the filial piety that was in him.

It is perhaps due to some extent to this circumstance that Jones was so perfectly matched with his times. It used to be said that, unlike Darwin, Freud never found his Huxley. Time has shown this to be a false judgement. Looking over the record of Jones's early academic training and of his later work as psychoanalytical leader, it is difficult to withstand a feeling of scientific predestination. There have been many gold-medallists whose career has stopped short in the graduation hall. In the case of Ernest Jones his early career, distinguished as it was, was but a preparation, a disciplining and co-ordination of his many faculties. So that when the turning-point of his life came and he fell under the personal sway, inspiration and encouragement of Freud he was already fully equipped to play the part of Adjutant-in-chief to a scientific movement which, owing largely to his initiative and good husbandry, was to become intercontinental in range.

This was perhaps one of the most constructive of his many services to psychoanalysis. Like all new and fulminating sciences, psychoanalysis is given to fissional emanations. Being in essence a psychology of conflict, it contains within itself the seeds of its own disruption. And it was due mainly to Jones's unflagging work in its diplomatic and administrative fields that its structure and functions have been preserved more or less intact. During eighteen years of official collaboration with him as a sort of sub-lieutenant, a period during which psychoanalysis was disturbed by powerful

cross-currents, both domestic and international, I never knew his judgement to falter or his diplomacy to fail in its ultimate aim of holding psychoanalysis together.

The task of defending psychoanalysis against attack called for quite different qualities. Diplomacy and polemic are refractory bedfellows. To misconceptions based on hostility Jones gave no quarter; and being a gifted controversialist with a quick eye for the soft underbelly of an argument and a sharp edge to his tongue, he soon instilled a wholesome respect in those who ventured to cross swords with him. Although responsive to the good opinion of the discerning he was proof against the unpopularity which an unusually able dialectician cannot escape when he espouses an unusually unpopular cause. But this was not, as was sometimes alleged, a proof of lack of feeling; Jones brooked and withstood unpopularity for the simple reason that he held the good name of psychoanalysis to be more important than success in the arts of professional diplomacy.

On the other hand, to the needs of the *bona-fide* student he was readily responsive. The gearing of his mind was essentially expository and a lengthy and voluminous record of didactic achievement in lecture and essay bore witness to his gifts as a teacher. The range of his knowledge was remarkable. There was no aspect of psychoanalytical theory and practice in which he was not thoroughly at home or which he could not conduct with admirable lucidity to tyro or colleague. He was in fact the best conductor of new ideas I have ever met, with the possible exception of Bertrand Russell. These gifts were displayed at their best in the Presidential Chair of the British Psychoanalytical Society which he occupied for quarter of a century. There can indeed be few of that fast-diminishing group who joined it in the early twenties who did not realize that throughout the first decade of its existence Ernest Jones was in fact the Society and that to the end of his life he remained the fulcrum by which psychoanalytical influence was brought to bear on British psychology and psychiatry.

The same comprehensive range of interest,

and indeed the same didactic tendency, were manifest in his innumerable research papers, whether these were concerned with pure or with applied psychoanalysis. Jones was much concerned to knit together the framework of psychoanalytical thought and was both ready and able to apply Occam's razor to any formulations that appeared to him to obstruct this aim. He was, by the same token, ready and able to fill many of the then existing gaps in psychoanalytical knowledge. Perhaps the most remarkable of his labours in this direction is preserved for us in his biography of Freud. For this was not just an exercise in the re-animation of Freud's life and character; it was also, one might say, a biography of Freud's thought, the best and most sustained exposition that exists in any language.

These then are the three pillars on which the psychoanalytical fame of Ernest Jones will surely rest: first, that he bore the brunt of defending Freud's discoveries against hostile erosion and domestic dilution; secondly, that by his teachings and researches he consolidated and buttressed the structure of psychoanalytical theory; and, thirdly, that through his talent for leadership, he saved the psychoanalytical movement from disruption. To these ends he brought an amazing combination of skills as teacher, translator, research fellow, writer, editor, administrator, advocate and entrepreneur.

But although this brief compendium of talents accounts for the reputation he deservedly acquired during his lifetime, it does not convey or explain the aura of distinction that enwrapped his personality. To understand this quality of greatness we must know something of the mainsprings of his character and of the regulatory mechanisms that controlled them. For there can be no doubt that successful resolution of conflict is, if not indeed one of the hallmarks of greatness, at least one of its prerequisites. In one of his Centenary Addresses, Jones himself said 'one cannot understand Freud without laying stress on the fact that he was a Jew'. Following this hint I would say that one cannot understand Jones unless one remembers that he was a Welshman born and bred. Essentially passionate in

temperament, deeply moved by experience, sensitive to a degree that sometimes created the impression of touchiness, combative in spirit and carrying an almost explosive charge of mental energy, it is clear that the touchstone of his greatness lay in the extent to which he was able to transmute his turbulence of spirit into disciplined thought and action. That in the long run psychoanalysis provided him with a sublimated outlet for his daemon was evident to anyone who knew him intimately. Psychoanalysis was for him more than a profession, a cause or a crusade. It laid the foundations for his adult Ego-ideal. It was as it were a hornbook for his personal life and character. Jones was well aware of this private debt he owed to Freud and to Freud's teaching; and in his professional life he repaid that debt to the full.

It remains for me to say how fitting it is that tribute should be paid to Ernest Jones during the Proceedings of the Medical Section of the British Psychological Society; and how much I appreciate the invitation to do so. Jones was a founder-member of the Section, contributed generously to its activities, and acted on the Advisory Board of the *Journal*. Not long before his death, during what proved to be our last meeting, I discussed with him amongst other things the function and future of this Section. I found him as keenly interested as ever in its welfare. His parting remarks on the subject were characteristic of his outlook. 'There are not', he said, 'many Societies in Britain in which Clinical Psychology can breathe freely. I hope', he added, 'the Medical Section will continue to perform this respiratory function'. Nothing, I am certain, would have gratified him more than to feel that the forum which he was partly instrumental in building up should remain, as it were, a spiritual platform for those who have this in common, that they study the psychology of suffering. For to the end of his professional life Ernest Jones held tenaciously to the view that what distinguishes medical psychology, and to some extent sets it apart from other branches of mental science, is its concern with the wells of mental pain.

CLINICAL PROBLEMS AND EXPERIMENTAL RESEARCHES*

By D. RUSSELL DAVIS†

In his endeavour to explain the clinical problems posed by disorders of mental activity and behaviour, the medical psychologist to-day may turn to psychological theories which have been elaborated in the laboratory. Yet so far these theories have exerted little influence either upon medical psychology or clinical practice. This might seem to be a strange state of affairs, but one reason for their relative failure to do so is obvious. Their applications to clinical problems have not yet been worked out in sufficient detail. Efforts to apply them have been made mainly, although not exclusively, by laboratory workers, and have proved abortive, at least partly, because they have not been supported by an adequate analysis of the clinical problems.

Let us take Pavlov's conditioned-reflex theory of mental disorders as an example. It is based upon the production of disorders in animals in the laboratory under conditions which have been carefully defined experimentally. These disorders show many interesting resemblances to those arising in human patients and may be regarded as homologous. Pavlov first formulated a theory to account for them more than forty years ago, and laboratory work has continued ever since, with the result that the theory is now a refined one. Yet it is still largely an academic theory, at least in Great Britain, for in contrast to the wealth of precise systematic observations made in the laboratory there is a paucity of relevant observations made in the clinic.

There have of course been many attempts to explain clinical observations by reference to Pavlovian theory, first by Pavlov himself and

his collaborators, e.g. (Pavlov, 1941) and later by numerous others, both experimental psychologists and clinicians. One of the most elegant and interesting essays of this kind is Sargant's account of acute combat neuroses examined by him at the time of the campaign in north-west Europe (Sargant & Shorvon, 1945). Pavlovian theory guided Sargant to some extent in his choice of methods of treatment, although he also depended upon 'excitatory abreaction', the rationale of which is largely due to Breuer and Freud—an example of the blending of two very different theories. Like many others of the same kind, Sargant's essay consisted, however, in no more than the description of abnormal responses in Pavlovian terms—mere translations of clinical jargon into laboratory jargon. He did not succeed in evaluating deductions from Pavlovian theory by the systematic assembling of clinical observations.

Eysenck (1957) has recently advanced further. In his latest monograph *The Dynamics of Anxiety and Hysteria*, he devotes the final chapter to a discussion of the applications of Pavlovian theory in the treatment of neuroses, but one cannot fail to be struck by the sketchiness of this discussion on any standards, and especially so if one compares it with the thorough examinations of laboratory experiments contained in the other chapters. Cautious and critical in his assessments of the results of laboratory experiments, he is reckless in his acceptance that methods of treatment based upon Pavlovian theory are both effective and rational. Thus he justifies his thesis by citing two remarkable cures when treatment was directed to the removal of the single salient symptom through conditioning procedures. One was in a case of urinary frequency, the other in a case of tics. In both, conventional methods of psychotherapy had failed. Other authors have claimed similar successes in the

* Address from the Chair, to the Medical Section, British Psychological Society, on 22 January 1958.

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treatment of such symptoms as enuresis, stammering, writer's cramp and alcoholism.

As it stands Eysenck's thesis is open to the criticism that it remains unproven that Pavlovian methods are generally more effective than the many other methods, some rational and others not, which have given impressive results in practice. He cites two successful cases, but he does not tell us the number of cases in which Pavlovian methods have been tried and failed. Cases of urinary frequency and tics are common, and a more extended trial should have been practicable. Clinical experience teaches scepticism about claims of superior success in the treatment of all the neuroses which have seized the interest of Pavlovian theorists. Hopes of an advance have often been raised and then disappointed. Nocturnal enuresis is notorious in this respect. Is the 'bell' method of treatment, about which he has expressed such enthusiasm elsewhere (Eysenck, 1953), and which, following Mowrer (1950), he justifies by reference to Pavlovian theory, any more successful than its predecessors or successors? Such evidence as there is is equivocal.

This criticism is of minor importance. Eysenck is engaged primarily in working out through laboratory researches the principles which should guide treatment. It is then quite legitimate to base arguments upon evidence obtained from two cases, or even from a single case, if, as in his work, the evidence has been obtained deliberately, in order to test specific hypotheses. In any event the success of a method of treatment is in general a poor test of the hypotheses which give the method its rationale, for there are usually many other factors which decide the outcome of treatment. If these are favourable, the outcome may be a cure, although the hypotheses are false. On the other hand, treatment may fail even though the hypotheses are valid.

There is a more important criticism to be made, which partly explains why Eysenck's, like other clinical applications of Pavlovian theory has achieved limited success. The researches he describes consist in making deductions from a laboratory theory and trying then

to discover whether they are valid by making appropriate clinical observations. In this way he provides a theoretical explanation of the observations. But, although the problems to be resolved are clinical, the hypotheses, the ideas, which govern the researches are formed in the laboratory. In consequence, the laboratory theory decides to which symptoms attention is devoted, and these symptoms are in general not those to which clinicians attach major importance. Furthermore, those who adopt this approach tend to accept too simple a version of the clinical problems; this is especially so when the theory is as narrow as is Pavlovian theory.

Nevertheless, attempts to apply laboratory theories in the clinic should not be disparaged, for they help to build a bridge from laboratory to clinic. Starting with a now substantial, although incomplete structure at the laboratory end, they throw lines across the gulf which separates laboratory workers from clinicians. There is, however, the complementary and perhaps for us the more important task of building a sound structure at the clinic end. A start has of course been made, and lines have also been thrown from the clinic to the laboratory, but there is an urgent need that clinicians should revise their descriptions of clinical phenomena, and especially of those aspects of their cases which they regard as of greatest importance, and should put them into a form which facilitates explanation in terms of laboratory theories.

Working hypotheses should be formulated as inductions from clinical and not laboratory observations, and deductions from them should be tested by making further clinical observations, but they should also be expressed, so far as is possible, in the terms of laboratory, or general biological, theories. Clinicians may be eclectic in their choice of theories, but they should try to define their problems in such a way as to invite laboratory researches. If they achieve a measure of success in this, they will provoke laboratory workers to undertake new studies, with the purpose of extending existing theories so that they can more readily be

applied clinically. It is remarkable, indeed deplorable, how little has been the influence which clinical problems have so far exerted upon laboratory psychologists in their choice of researches.

With this failure in co-operation in mind, I shall discuss how clinical problems can be put into a suitable form, which relates them to laboratory experiments. I shall take, for the purpose of illustration, a particular set of clinical problems, which I have chosen, not only because their importance is obvious, and because they have seized my interest, but also because they have characteristics which I believe are shared by the major contemporary issues in medical psychology. Thus they raise questions about the processes concerned in the production of symptoms, and about the processes concerned in psychotherapy, and they cannot be adequately discussed nowadays without reference to laboratory experiments.

* * * *

Breuer and Freud wrote in 1893: '...each individual hysterical symptom immediately and permanently disappeared when we had succeeded in bringing clearly to light the memory of the event by which it was provoked and in arousing its accompanying affect, and when the patient had described that event in the greatest possible detail and had put the affect into words' (Freud, 1955, p. 6).

This excerpt describes clinical observations. It also adumbrates certain explanations. In the passage from which it is taken, Breuer and Freud go on to argue that the removal of hysterical symptoms depends upon the revival of the memory of the traumatic experience, and that with this revival three further processes are associated: (1) *The abreaction of the 'strangled' affect.* The affect hitherto 'strangled', or 'pent up' in an earlier translation of the word *eingehemmt*, is aroused and discharged. 'Recollection without affect almost invariably produces no effect', they said. (2) *Verbalization.* The affect is put into words; the original process is given verbal utterance. (3) *The correction of the idea.* The idea, which

was not abreacted in the first instance, is subjected to associative correction by introducing it into normal consciousness or is removed through the physician's suggestions.

The suppositions represent attempts to formulate explanatory hypotheses, which have been restated on numerous occasions, of course also by Freud himself, in many forms and with different amendments and different placing of the emphasis. Everyone who has written on psychotherapy in this century, or for that matter who has practised psychotherapy, has had to make judgements on their validity. The authors of a recent book, for instance, sum up their views in these words: '...to become conscious, to verbalize, and to communicate are integral parts of the therapeutic process' (Foulkes & Anthony, 1957, p. 227).

Yet we lack systematic evidence on which to base judgements, and are little better placed today than were Breuer and Freud sixty years ago to draw firm conclusions about the relative importance of the four processes. Progress in resolving the many problems contained in the original suppositions has been slow. The most likely reason is that the problems have not yet been formulated in suitable terms, which are in accord with contemporary theories. That is, the right research questions have not yet been put. Only if we put the right questions does nature unveil her secrets.

It is generally assumed that psychotherapy based broadly on the suppositions enunciated by Breuer and Freud is effective in a wide range of cases. This assumption hardly comes into doubt, for there are now many well-attested cases in which clinical improvement has taken place, or a hysterical symptom has disappeared, in close relation to the recall of traumatic experiences. Such cases are commonplaces, and the assumption has been little if at all weakened by Eysenck's (1952) much discussed failure to demonstrate statistically in large pooled samples that 'psychotherapy facilitates recovery from neurotic disorder'. Yet the question, 'Is to promote the recall of traumatic experiences an effective method of treatment?', is probably too simple, for less importance

tends now to be attached to the reviving of memories directly, and more to the establishing of conditions in which the defences which prevent recall are abandoned. Nevertheless, it is still supposed that their recall plays a part, although a less central part, in treatment, and one may ask, as Breuer and Freud did: Through what processes does their recall bring about its beneficial effect? However, it may well prove more profitable to ask: 'What part does recall play in the recovery, or, more generally, in adaptation to traumatic experiences?'

* * * *

Let us consider therefore, the first of Breuer and Freud's hypotheses as an answer to this last question. They said, in effect, that the recall of traumatic experiences is adaptive because it provides an opportunity for the discharge of the excitation which has hitherto been denied discharge in the normal way, and which has become pent up; the discharge occurs, they supposed, in accordance with the 'tendency to keep intracerebral excitation constant' (p. 197).

This hypothesis depends upon the ideas that excitation is generated in the central nervous system by traumatic experiences, has normal channels of discharge in adaptive activity, can also be pent up, i.e. stored, and can be discharged vicariously through the production of neurotic symptoms. Similar ideas enter into Freud's earliest explanations of the symptoms of anxiety neurosis, when he supposed that they represent the vicarious discharge of excitation which has been denied discharge in normal sexual activity.

These ideas have also provided a rationale for the method of treatment now known as 'excitatory abreaction', in which, in its modern form, drugs such as ether by inhalation or methedrene intravenously are used to bring about an especially violent display of emotion, while memories of traumatic experiences are being revived. This method came back into fashion during the second world war and still has its advocates (e.g. Sargant & Slater, 1954; Palmer, 1958), who claim that the more violent

the display of emotion the greater the benefit especially if anger rather than terror is expressed. Full recall of the experiences is said to be unnecessary; these tend to be as much re-enacted in movement as verbalized. What is verbalized tends to be fragmentary and incoherent.

One may evaluate a hypothesis of this kind by considering whether clinical observations, on balance, speak in its favour. In support of the view that emotional arousal and discharge brings benefit might be cited a number of everyday observations and also, more crucially, Breuer and Freud's assertion, if it is accepted as valid, that recollection without affect almost invariably produces no effect. In refutation it might be mentioned that in battle dreams the recall of traumatic experiences is accompanied by intense emotion, but that these dreams are not immediately followed by any clinical improvement. Moreover, when recall occurs under the influence of drugs, the results obtained with sedative drugs, which keep emotional arousal to a minimum, are as good or better, it might be claimed, than those obtained with drugs which produce excitement. It would be fair to say, I think, that the consensus of opinion is now opposed to the method of excitatory abreaction, but that the evidence is inconclusive.

The hypothesis may also be judged by inquiring whether the ideas it contains are in accord with existing psychological or physiological theories. One must conclude that they are not. As we have noted, the central ideas are that affect not abreacted at the time of the trauma is pent up, although it may leak away in the guise of symptoms, and is ultimately released when the trauma is recalled, days, weeks, years or decades later. These processes are physiological rather than psychological, but they do not correspond to any others that are known, although it would be easy to devise a model of the brain which would serve them, with electrical circuits, condensers and the like, or tanks, pipes, valves and overflows. Similar ideas have reappeared frequently, however, during the last fifty years in the explanations

proffered for many diverse phenomena by both experimental psychologists and clinicians, and are still prevalent. They enter, too, into some ethological concepts, such as displacement activity, in so far as this has been attributed to the 'sparking over' of energy derived from one drive 'denied discharge through its own consummatory act' (Tinbergen, 1952) to a system of responses appropriate to another drive.

Freud himself did not at any time discuss the possible mechanisms in detail, although he alluded to them in 1893, and again many years later in his monograph *Beyond the Pleasure Principle*. They have never been defined clearly, nor have they been given an experimental basis. If it is to be retained at all, therefore, the 'abreaction' hypothesis requires to be expressed in a form more in keeping with contemporary theories.

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One may look at the clinical problems from another point of view, although still regarding the revival of memories as forming part of the processes of adaptation to traumatic experiences. In doing so, one must keep in mind what is observation and what supposition. It is observed that when traumatic experiences are recalled there tends to be emotional arousal. It is supposition that this is due to the release of pent-up effect. An alternative psychological explanation is not difficult to find.

It might be possible to resolve the problems by making experiments in which human subjects are exposed deliberately to situations causing them distress and frustration. Such experiments are not impracticable and in some circumstances are quite proper if due care is taken, but they meet with many difficulties which reduce their value. They are premature until a theory has been evolved in broad outline which indicates what kinds of observations are most needed on human subjects.

There are now many series of experiments on animals which have contributed to a theoretical description of the processes of adaptation to traumatic experiences. As examples may be cited Masserman's (1943)

experiments. He exposed cats to an air-blast, which was made to impinge on their necks just as they were about to take food from a food-box in response to a signal. This was the traumatic experience. There were three phases in its effects: (1) *The immediate reaction*. The cats showed a general disturbance in behaviour, with, amongst other things, excitement, restlessness and agitation and the typical somatic concomitants of anxiety. (2) *Defensive avoidance*. They withdrew from the vicinity of the food-box, or from the cage, and resisted strongly any efforts made to bring them back into it. (3) *'Working through'*. Under some circumstances, if they were not forced, and were given a degree of control over the situation, the defences were abandoned, and they made tentative approaches to the food-box and began feeding again, despite the air-blast, to which they gradually became accustomed. If we describe this sequence of events as fright, flight and return, it is obvious that it is a common one in all species of animals.

The excitement and emotional arousal provoked by the traumatic experience persisted for a while after the cats were allowed to leave the cage and then gradually subsided. It was provoked again whenever the feeding signal was given, or if they were put back into the cage. If they were forced back into the vicinity of the food-box, they displayed intense emotion and distress, which increased the nearer they were brought to it, and which subsided again as soon as they were allowed to withdraw from it.

An explanation for the excitement and emotional arousal observed in these experiments can be found in the theories, elaborated in laboratory experiments on animals and man, which deal with secondary drives, danger signals, instrumental avoidance training and the like. It is then supposed that the food-box and cage acquire for the cats the significance of danger because of the experience of the air-blast; the feeding signal also becomes a danger signal. The excitement and emotional arousal are explained as being a reaction to danger, being evoked whenever the danger appears

imminent and subsiding as it recedes, and representing poorly organized attempts to gain control over it. The cats defend themselves by avoidance, i.e. by staying out of the danger situation. While they do so, their attitudes towards it remain fixed, because the defence precludes the modification of the attitudes through learning from further experience. If they return to the food-box, their attitudes change quickly.

The lesson of these and other experiments puts us into a position to re-interpret some of Breuer and Freud's observations, in much the same way as Freud came to re-interpret them on clinical grounds. We may regard the revival of memories of a traumatic experience as being a symbolic return to a danger situation, and the repression of the memories as being the counterpart of the defensive avoidance shown by the cats. While they are repressed, the memories remain unmodified by further experience. The affect aroused when the traumatic experience is recalled is not the stored product of the original experience, but an immediate reaction to the danger represented by the memories.

This re-interpretation carries implications for the treatment of human patients, for we may now regard the objective of treatment as being to bring the patient back into the danger situation, actually or symbolically, under conditions in which attitudes towards it can be modified by further experience. To achieve this result, the defences have to be nullified. We can then go on to ask whether this new learning is more likely to take place when emotional arousal is maximal, as it is when the return is forced and in the method of excitatory abreaction, or when it is minimal, as when the cats, or the patient, are allowed to work through gradually.

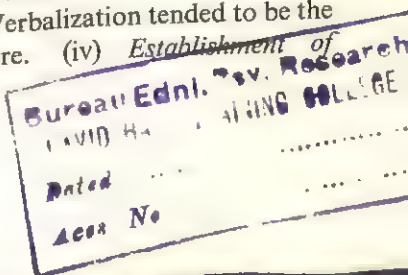
The theoretical answer is reasonably certain, for there is laboratory evidence to show that adaptation to complex situations, or the acquisition of skills, occurs most readily in man and animals when emotional arousal is low. Much of this evidence comes from experiments in which have been studied the effects of in-

crease in drive strength upon the learning of tasks of different degrees of complexity. Laboratory experiments have also shown that new responses learnt when drive strength is high tend to become stereotyped and fixed. That is to say, the best results are likely to be obtained in treatment if anxiety is kept to a minimum.

Laboratory experiments using human subjects have a certain contribution to make at this point. As examples I shall take my own wartime experiments with the Cambridge Cockpit (Davis, 1957), although they were carried out with other problems in mind. In these experiments pilots were put into a machine over which they found themselves unable to maintain control. This proved a distressing, traumatic, experience, and they became intensely anxious. In order to describe the effects on these human subjects it is necessary to expand the three-phase scheme into which the results of Masserman's animal experiments were fitted, and I have drawn up a scheme of six phases.

There were two phases in the immediate reaction: (i) *Over-activity*. The subjects became excited and very active, showing persistent striving to gain control over the machine. Their behaviour gradually became disorganized, and aggressive and regressive behaviour appeared. There was considerable disturbance in bodily functions. (ii) *Psychological withdrawal*, or 'inertia'. Responsiveness to the demands made by the situation declined. The disturbance in bodily functions tended to subside. Responsiveness, reduced while the subject stayed in the situation, gradually recovered after he went out of it.

There were four phases in the recovery after the laboratory test was terminated: (iii) *Pre-occupation and fixation of memories*. The subjects showed a compulsive preoccupation with the experience and tended to talk freely and repetitiously about it. Their minds dwelt on it. This represented the continuation of attempts to gain control over the situation, but now in symbolic form. Verbalization tended to be the dominant feature. (iv) *Establishment of*



defences. These took many forms. Repression was one form, compensatory gratification another. Some subjects sought distractions, some went drinking, others slept, others deprecated the test. (v) *Abandonment of defences*. There was a gradual weakening or shifting of the defences, with some recall of the experience in a more or less distorted form in day-dreams or dreams. (vi) *Working through*. Memories of the experience were more freely revived, gradually became modified and faded.

This scheme gives a general description of the observations made in my experiments. It might also be extended to cover clinical observations. If this is to be done, it should cover the known clinical facts. These are not in conflict with it, I think, but to show that this is a correct conclusion calls for a critical review of a wide variety of clinical observations. It is unnecessary to present such a review here. So far as I know, there is no body of systematic data which could be used to confirm or refute the hypotheses contained in the scheme. Suitable data might, I thought, be found in accounts of human reactions to disasters, such as transport accidents, earthquakes and the like, but the yield from a search through the literature has been disappointing.

A recent account by Friedman & Linn (1957) is of some interest. They had the opportunity to examine some of the survivors within a few hours of the *Andrea Doria* disaster. The emotional state of the survivors, they write, could be divided into two phases: *initial psychic shock*, and *recovery*. In the former, which corresponds to the phase of psychological withdrawal on my scheme, the survivors acted as if sedated, and were passive, compliant and retarded; in some instances they were amnesic for data of personal identification. In the latter, which corresponds to the phase of pre-occupation and fixation of memories on my scheme, they showed a great need to tell their story over and over again, usually with identical detail and emphasis. These repetitive narratives were similar to the repetitive dreams of traumatic neurosis. 'Each represents', the authors assert, 'a reliving of the trauma, as

part of an attempt to master an experience that had proved overwhelming'.

There are various other clinical data which would be worth while collecting systematically. For instance, I have collected data intended to answer the question: What is the fate of the memories of traumatic experiences which emerge during psychotherapy, after the patient has recovered? One would expect that they would have become assimilated into other related experiences, i.e. associatively corrected or 'schematized', and would be incapable of independent recall in anything like their original form. My findings correspond to these predictions in the majority of cases, but there are a substantial number of cases in which fixed, stereotyped memories are readily revived after recovery; there is then no excitement or emotional arousal. These memories are similar in character to those recalled during the third phase. It seems likely, therefore, that assimilation into schemata is one way, and transformation into stereotyped verbal terms is another way in which memories of traumatic experiences are divested of their pathogenic effects.

Bartlett's (1932) experiments have told us a great deal about the way in which memories become modified when they are revived, and the many changes which they undergo as they become incorporated into schemata, and I have reported experiments which show how the memories of one experience may be modified by other related experiences (Davis, 1951). Very little is known, on the other hand, about the processes or effects of the stereotyping of memories, although Bartlett demonstrated that frequent repetition leads to the stereotyping of reproductions, which thereafter suffer little change. It is widely believed that anxiety arising from a painful experience can be alleviated by writing down a verbal account of it or by representing it in a picture. It might be argued that to do either of these things is equivalent to the stereotyping of it. Possibly the essential process is the organization of unclear or incoherent images. Further experiments are needed to define the part played by verbalization in the recovery processes. They

might make use of the 'interruption' method, introduced by Zeigarnik (1927).

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I have nearly come to the end of my discussion of the part played by the revival of memories in adaptation, on which I embarked in order to prepare for some comments on the aims and methods of medical psychology.

Its problems mostly arise out of the symptoms, the examination and the treatment of patients. The impetus to research comes from the urgent need to improve the methods of psychotherapy. Yet the elaboration of theory must come first, application in treatment second. The first task is to study the processes of reaction and adaptation to untoward circumstances, and the failures of adaptation, which go on in patients whether or not they receive treatment. Only when we know something of the natural processes of recovery, can we usefully discuss how we can best assist nature, and decide when and with what objectives we should interfere therapeutically.

The theory decides the objectives of treatment. If we are to treat a patient in the third phase on my scheme, we would be able to do so more effectively if we knew more about the part played by verbalization in bringing about recovery. We can only surmise that the essential process lies in the fixation of memories or the organization of unclear thoughts. We do not know what importance lies in the communicating to, or the sharing with, the therapist. Again, we could treat a patient in the fifth phase more effectively if we knew more about the conditions in which defences like repression are abandoned spontaneously, and memories are then revived and assimilated.

It seems likely that repression weakens when the recall of the traumatic experience has become less threatening and therefore provokes less anxiety. It has been stated, for instance, that pilots who have sustained flying accidents more often recall them in dreams after they have been grounded, and similarly that coal miners more often recall disasters in the pit after they have changed their occupation. In

these cases repression has become less necessary as a defence, it may be supposed, because the external circumstances provide some protection. In most cases the recall of a traumatic experience also threatens the patient's conception of himself as a good person. Thus the content analysis of statements made by patients in sessions in which they recall traumatic experiences show that they tend to make a relatively large number of statements ascribing good qualities to themselves. In this way repression is replaced by another method of defence. One might well go on to argue that the general reason why repression weakens during psychotherapy, and painful memories are revived, is that, feeling safe with the therapist and assured of his sympathy and respect, the patient has a new defence against the threat to his conception of himself.

I have been describing what may be regarded as the natural history of recovery from painful experiences. Medical psychology is indeed at the natural history stage of development as a science; that is, at the stage of making general descriptions of the processes of adaptation, just as fifty years ago pathology was engaged in working out descriptions of key processes like inflammation. Medical psychology at present depends on qualitative studies, which must necessarily precede quantitative studies. It is not yet at the stage at which rigorous proof or precise definition can be attempted.

Nevertheless, its methods are experimental. By experiment is meant no more than observation made with some preconceived idea, or made with some purpose. That is, observations are planned so that they can be used to argue from the particular to the general, or to infer a hypothesis by induction. From the hypothesis is made a new deduction, the validity of which is tested by making further observations. If he is to conform to these rules of procedure, the medical psychologist plans his observations in advance, relates them systematically to the hypothesis which he is examining, makes them deliberately and records them accurately (Fisher, 1949, p. 8). If they do not bear out his deductions, he rejects or amends the

hypothesis. In other words, 'he puts a question to nature, and when nature replies, he holds his peace, takes note of the answer, listens to the end and submits to the decision' (Bernard, 1927).

The rules restrict in certain ways only, the kinds of observation that may be used as evidence. They do not demand, for instance, that observations should be quantitative. They do not disqualify reports of thoughts or feelings. They do not insist on elaborate methods or complex experimental designs. What contrivances are required depends upon the question which the experiment is intended to answer. Often the best question is the one that can be answered with least contrivance. To imitate the specialized procedures and designs of some contemporary researches in psychological laboratories is not necessary. Models for researches in medical psychology are at least as likely to be found in the tradition of experimental medicine.

By far the most important stage in every

research is deciding what question to put, and in what form to put it. As in the parlour game 'Twenty Questions' one good question gains more than several poor ones, which may add to the confusion. Once the question has been formulated, it can be decided whether the answer should be sought through experiments on animals or on human subjects or patients. That is, there must be free and frequent passage over the bridge between laboratory and clinic. Thus laboratory studies both on animals and human subjects, and the systematic observations of animals in their natural habitats, as well as clinical observations, may all contribute to a description of the processes of recovery from a traumatic experience, as I have tried to show.

Finally, we should not expect that experimental researches in medical psychology will lead to new discoveries. All that they are intended to do is to confirm and then refine, hypotheses already accepted as reasonable on clinical grounds.

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THE CONCEPTS OF SUBJECT AND OBJECT IN PSYCHOANALYSIS

BY MICHAEL BALINT*

Both *object* and *subject* are not quite exact and rather aggressive renderings of much gentler, unaggressive Greek words. The Greek originals were created by the philosopher grammarians of the Stoic school, rather late in the day, when almost all the classical Greek literature as we know it was already extant. The Homeric epics, the tragedies of Sophocles, Euripides, and Aeschylus, the comedies of Aristophanes, the histories of Thucydides and Xenophon, all the beautiful poetry of Pindar, Sappho, Anakreon, etc., even the dialogues of Plato, had long been written when Aristotle, and after him the Stoa, started to tidy up our ways of thinking and speaking about things and events. For them any statement—predication or proposition—consisted of two elements: (1) something essentially constant, to which (2) something accidental or peculiar was attributed. The something essentially constant about which the statement was made was called by various names according to the context; one of them was τὸ ὑποκείμενον which literally means 'that which lies under'. The Latin translation substituted 'being thrown' instead of 'lying', and created 'subject', literally meaning 'that which is thrown under' (an action or a statement). The other person or thing to which the action or statement extends was called τὸ προκείμενον or τὸ ἀντικείμενον meaning literally 'that which lies against or athwart'. This was translated into Latin as 'object', in the same aggressive way, meaning 'that which is thrown at or athwart' (an action or a statement).

Most European languages have simply borrowed the Latin terms, thus escaping the necessity of creating their own. The only two exceptions I know of are German and Hun-

garian. The first translated *object* literally to 'Gegenstand', that which stands against, i.e. resists, while the latter calls it 'tárgy' which originally meant 'target', and it is possible that the two words have the same root. Moreover, both *object* and *subject* have further aggressive connotations. According to the *Concise Oxford Dictionary*, 'to object' means 'to adduce as objection; state as damaging fact to or against a person, etc.; state objection; feel or express disapproval; have objection or dislike to, etc.' 'To subject' means 'subdue, expose, treat'. 'Subject' as an adjective means 'under government, not independent, owing obedience, in subjection, liable or exposed to, etc.'.

Thus, *object* seems to have two inter-related meanings. In the one it denotes the *target*, determining by its attraction the direction of the action described by the verb of the sentence. It was this meaning that prompted Freud to choose this word to describe an important aspect of the instincts. In his *Three Essays* (Freud, 1905) he tried to tidy up the field of the sexual instincts using three criteria for classifying them. These are the aim, the object and the source of an instinct. The *source* is the part of the body in which the instinctual urge is felt to originate; the *aim* is the act towards which the instinct tends; and lastly, the *object* is that part of the external world (usually a person) or of one's own body from which the sexual attraction proceeds.

The other meaning of *object* seems to be *obstacle in the way of the action*, in fact a resistant obstacle that has to be negotiated. If I remember rightly A. McLeod (1951) called our attention to the idea that perhaps our very first perceptions about objects may be those of resistance, i.e. something firm against which we may pit our strength, either successfully or unsuccessfully. This conception is certainly in harmony with the cluster of associations surrounding the word 'object'.

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According to this second primitive idea, which is however still rampant even in our adult thinking and feeling, objects are something firm, sharply contoured and resistant. I have in another paper (Balint, 1952) discussed the fate of this idea in artistic creation. We may assume that an eternal ambition of art is to represent nature or life (which are meant to include the inner world of the artist) as faithfully and sincerely as possible. For quite a time nature—or life—was conceived as a collection or conglomeration of solid, separate, clearly defined and sharply contoured entities, called objects. Science followed suit, and for some time conceived both the physical and the chemical worlds as consisting of firm, sharply contoured objects, mechanical points and molecules or atoms. It is easy to show that this picture of the world is based partly on a psychological process: projection. We conceive the objects, the ultimate constituents of the world, as we wish to see ourselves, or perhaps even as we really see ourselves: firm, unchangeable, indestructible, in fact, eternal. Since on this occasion we are concerned only with the primitive conceptions of subject and object, I will not follow up the various changes that object representation has had to undergo in recent centuries, in science, literature or art. To sum up, the meaning of 'object' as a resistant obstacle seems to be more primitive than a target or an objective.

Liquids are not really objects, and gases certainly are not. It was a later, more sophisticated way of thinking that was able to integrate these two states of matter with the solids in the general conception of objects. When attempting to comprise solids, liquids and gases together, science and philosophy developed words such as *substrate*, *substance* and *matter*. The first two are Latin non-aggressive variants of the already quoted Greek original τὸ ὑποκείμενον. *Substrate* is a comparative newcomer to the English (though not to philosophical) language and means literally 'that which is spread under'; *substance*, which is well established, means literally 'that which stands or is under'. They both mean, more or less, the

essential nature underlying the accidental phenomena; or that which is regarded as supporting the attributes and accidents, that which receives modifications but is not itself a mode, that in which accidents or attributes inhere. And lastly, *matter* derives from a common Indo-Germanic root denoting *mother* (this is, of course, a much simplified description of an immensely complex topic).

Here we encounter our first problem. Parts of the external world which are felt to be firm, resistant and sharply contoured are called by a special, somewhat aggressive name—*object*, which suggests both resistance against our wishes and an aim or target for our strivings. Other parts of the same world, which are not solid, do not resist much and have no real contours, are called by non-aggressive names, such as *substance*, *substrate*, both showing similarity to *subject* denoting ourselves. A third, very generally used, word describing these not so sharply contoured, less resistant parts of the world derives from a root, denoting *mother*. The inescapable inference is that at one time there must have been a harmonious mix-up in our minds between ourselves and the world around us and, that our 'mother' was involved in it. Though this mix-up strikes us as childish and primitive, we must admit that it preceded our 'modern', 'adult', or 'scientific' picture of the world which, so to speak, grew out of it, and undeniably some of its primitive features were carried over into its later forms.

Before discussing what psychoanalysis can contribute to the understanding of this development, I wish to mention that the physiological psychology of the senses is in full agreement with the idea of the secondary nature of objects. I think it is generally agreed that ontogenetically sharply contoured objects emerge only gradually out of a matrix—another suspiciously primitive word deriving from 'mother'. The two most important senses that provide the perceptions which form the basis for the discovery of 'objects', are sight and touch. Both of them are undeveloped in the first post-natal months as they need a considerable degree of muscular co-ordination to work

properly; it is well known that binocular vision does not exist in the first weeks. Moreover, both sight and touch, together with hearing, are projective senses; they feel, place or construct the object outside the body, either at a distance (sight and hearing) or at its surface (touch).

The situation is utterly different with the two lower senses which are well, perhaps even fully, operative at birth. In their function there is hardly any projection; we feel smell and taste inside our body—in our mouth or in our nose; moreover, the sensations themselves, more often than not, have nothing to do with objects, only with substances. This gives us some idea as to how and why the mix-up between ourselves and the world around us has come about. Looking at it as a detached observer, we can recognize that it is based on an interaction between the individual and the external world; one may say that the world has intruded or penetrated into the individual's mouth or nose, and equally correctly, that the individual has taken in parts of the external world—penetrated into it.

A mix-up also occurs with the sense of temperature, though to a lesser degree. In fact it forms a transition between the lower and the higher senses, i.e. those based on mix-up and those using projection. Cold and warmth are felt partly as coming from outside, partly as a state of our own body or even of ourselves: we feel warm or *it* is warm. Concurrently any object, if it is recognizable at all on the basis of temperature sensations alone, is felt or construed only in a hazy and vague way.

This mixing up of the external and of the internal worlds is well known in psychoanalysis, and has been studied from various angles for various purposes, though a systematic study of this topic has not yet been undertaken. First, there exist a number of clinical phenomena in which the boundary between the two worlds becomes blurred or even disappears. These are—illusion and hallucination, confusion, fugues, depersonalization, and the toxic states caused by drugs. Then there are the two important dynamic processes: projection and introjection. We

tend to behave as if everything good were in us—introjection, and everything painful or objectionable in the external world—projection. Though checked to a large extent by the function of reality testing these tendencies remain active as long as we live. True, they soon become complicated by secondary processes and the ultimate picture is anything but simple and transparent. In particular introjection is often followed by idealization of the introjects and by identification. We acquire our ideals by the first process while the second may transform an individual to such an extent that hardly anything genuine remains of him. A well-known example in academic circles is the faithful pupil who is but a second copy of his master.

All these processes—projection, introjection, idealization, identification—happen all the time, though their intensity varies within very wide limits. The psychoanalytic situation is an excellent setting for studying them, and in fact psychoanalysts have been the main research workers in this field. Although these processes are most important for our topic, as well as for psychoanalytic technique, I wish to confine myself to another field, the emergence of objects and the effect of this on the mind.

It may sound strange that this process can be observed in people in analysis for no matter whether they are patients or candidates who train for psychoanalysis, adults or children, they are all well beyond the time when objects were being discovered. This is true, but during psychoanalytic treatment a curious process takes place, called regression. Under the impact of the psychoanalytic situation—an essentially two-person relationship—very powerful emotions are set in motion in the patient or candidate. If the analyst keeps to the rules of his technique these emotions become more and more primitive, that is, they show a regressive trend. The emergence of the regressive trend is an undeniable clinical fact, and it is moreover present and active in every therapeutic relationship, from ordinary medical treatment, through Christian Science, faith healing, convulsive therapy, hypnosis, to classical psychoanalysis. The difference between

psychoanalysis and all the other therapeutic relationships is that in psychoanalysis this regressive trend is most carefully studied and examined both by patient and analyst.

There is some divergence of opinion among analysts about the dynamism of regression. Some maintain that it is a general human tendency and proper analytic technique refrains from interfering with its spontaneous progress. Others, I for one, think that regression, though a general human response, is actually elicited by a therapeutic relationship and especially by the analytic situation. The analytic situation is based on a partnership between two people, in which in many ways, only one of them matters; his anxieties, needs, interests are the only ones that count and must be attended to. The other partner, though felt to be immensely powerful, matters only in so far as he is willing to gratify the first partner's wishes and needs or decides to frustrate them. Beyond this his personal needs, interests and problems simply do not exist. Every therapeutic relationship contains elements of this basic pattern, and this fact is in my opinion the explanation of why some regression inevitably takes place during any treatment. As most treatments are short-lived incidents the influence of regression in them is not great, but the longer the treatment lasts the more important it becomes. These well-known observations suggest that something in the therapeutic pattern stimulates regression. In fact, experience shows that the nearer the pattern just mentioned is realized in any treatment, the further the regression will go.

Of course, this holds for psychoanalytic treatment. In some patients the pathological processes are fairly accessible, and their treatment can be terminated without much regression. In others, the pathological processes are too involved or only little accessible, and in consequence the treatment takes a very long time. Experiences with these latter patients supply much interesting material for the study of the mix-up between subject and object.

The common gist of these experiences is that all of us have a phantasy of a primal harmony which we feel ought to be our due by right, and

which was destroyed through our own fault, through the machinations of others, or by our cruel fate. It is impossible to get an accurate description of this state apart from that in it all of our wishes would be automatically satisfied, in fact we would feel no want. This harmony is the theme of a number of religious beliefs and fairy tales, and appears to be the ultimate goal of all human striving. This striving for a complete harmony between the subject and his environment may be approximated in our sexual life, in particular in its most intense phase (orgasm) and in all forms of ecstasy. Perhaps the most important quality of all these states—fairy tales, ecstasies, sexual orgasm—is an almost complete identity between the individual and his environment, i.e. between the microcosmos and the macrocosmos.

The fact that it is difficult to get a proper description of these states points to the possibility that they belong to a period in which words did not yet exist. In psychoanalysis the following three theories have been developed to explain such states; in some way they contradict, in another complement one another.

(1) *The theory of primary narcissism*, according to which all emotional interest was centred originally on oneself, and only later experiences forced or enticed the individual to detach some of his love from himself and turn it towards parts of his environment. (2) *The theory of absolute omnipotence*, a state which is thought to be a kind of second stage to primary narcissism. According to this theory the infant, provided he is nursed fairly efficiently, assumes that as soon as he experiences a need or a wish it will be immediately satisfied, because his wishes, or he himself, are omnipotent. (3) *The theory of primary object-relationship or primary love*. This maintains that a healthy child and a healthy mother are so well adapted to each other that the same action inevitably brings gratification to both. Good examples are sucking—feeding, cuddling—being cuddled, and so on. Thus for some time a healthy infant feels that there is no difference of interest between himself and his environment, i.e. that he and his environment are mixed up.

According to the theory of primary narcissism, in the first phases of life there is no experience of an external world, only of the self. The theory of primary omnipotence allows a hazy notion of something beyond the self, but not of harmony between the beyond and the self, only of automatic, instantaneous satisfaction. The theory of primary love presupposes the experiencing of an external world, but it assumes that there exists a harmony between the individual and his world, that there cannot be any clash of interest between the two. Obviously this state is nearest to the mix-ups discussed earlier in this paper. A good example in adult life of this primary state is our relationship to the air surrounding us. We use it for our own ends, inhale and exhale it, take parts out of it that we need and put other parts into it that we want to get rid of, without even noticing its existence. In fact it would be a somewhat comical question to inquire whether the air likes being used in this way. It must be there because we simply cannot live without it.

It will be noted that the air is not an object but a substance. There is, moreover, no need to define exact boundaries between the external air and ourselves. In fact, it would be hardly more than a play with words to ask, for example, whether the air in our lungs is part of ourselves or part of the external world. Obviously it is both—a mix-up. The same holds true of the contents of our bowels which are our 'inside' in the strictest sense of the word, yet in another way belong to the external world.

May I add that this neither-here-nor-there psychological state is reciprocated by the embryology and histology of the same organs. Both the intestines and lungs are lined with a special kind of epithelium which is contiguous with the skin that covers our body and which, though morphologically different, is embryologically derived from it. So here too we have a kind of mix-up between outside and inside.

Through clinical experience with regressed patients we have arrived at a primitive picture of the world in which (a) there is complete harmony between individual and environment, (b) the individual does not care and is not in a

position to say where he himself ceases and the external world begins, nor (c) can an external observer define exact boundaries. In the stage of development to which this refers, there are as yet no objects, although there is already an individual who is surrounded by and floats in substances without exact boundaries. The substances and the individual mutually penetrate each other in a harmonious mix-up.

This gratifying harmony is intermittently shaken by a long series of traumata caused by the emergence of objects. Unlike substances, objects are firm and sharply contoured; with them there is no more harmonious interpenetration, no more casual mix-up; exact boundaries must be accepted and respected. Clinical experience shows that people react to the discovery of objects in a wide variety of ways. In order to simplify this highly intricate subject I have isolated two types, although needless to say hardly any person will be found who entirely fits into either type. Still, if simplification is permissible, one may say that each of the two extreme types creates for himself a characteristic world. To characterize these two worlds let us turn now to a class of amusements in fun-fairs, consisting of swings, roundabouts and switchbacks, all of which are connected with some temporary impairment or loss of equilibrium, that is with temporary loss or impairment of security (cf. Balint, 1955).

Though many people show no marked reaction to these forms of amusement or are more or less indifferent to them, two extreme attitudes can be recognized. One type of person enjoys them immensely, seeks them out, and is willing to pay for them. The other type hates and fears them: they may even make him sick.

Reactions to similar impairments of security, or in fact to any danger to physical integrity, may be classified into the same two types. One type of person is stimulated by any such impairment or danger to acquire new skills for coping with it. People belonging to this type keep their eyes open, try to face the oncoming danger, that is 'stand up to it' and watch for possible clues to overcome it. People of the other type

tend to turn away, to shut their eyes and, with increasing impairment, to cling to a firm and safe object. First they use their hands, later they have to crouch or sit down, and finally press their whole body against whatever firm object is available.

From this point connexions can be established with certain forms of human activity referred to as 'thrills'. The constant pattern of these activities is somewhat like this—a man intentionally leaves the zone of safety and exposes himself to all sorts of danger and hazard in the conviction that his skill and his luck will be sufficient to enable him to return to security again. This group includes all kinds of racing, activities connected with high speeds, rock climbing, acrobatics, high-diving, expeditions into unknown lands—in fact every kind of unfamiliar pleasure. Since there is no place for dealing with these phenomena here, the only point I wish to mention is that in this field, too, we meet the same two types; one is desirous of these thrills—either as a professional, amateur or as a spectator—and enjoys them; the other avoids them if at all possible, and is contemptuous of them.

In order to describe these two utterly different attitudes towards the physical world around us, I have coined two words. Using as my model the word 'acrobat' which literally means 'one who walks on his toes', that is, away from the safe earth, I have coined 'philobat' to denote the people who enjoy thrills, roundabouts, and the like. The other type I call 'ocnophil', based on the Greek verb *ὀκνέω* which means 'to shrink, to hesitate, to cling'. An ocnophil is an individual who cannot stand swings and switchbacks, who prefers to clutch at something firm when his security is impaired, who, if at all possible, shuts his eyes or turns away from the oncoming danger.

Using these two words we can now describe the two different worlds. The ocnophilic world consists of firm objects to which one may cling, and which are separated by cold and horrid empty spaces. The ocnophilic individual lives from object to object, cutting his sojourn in the empty spaces as short as possible. Fear is pro-

voked in him by leaving an object and allayed by joining another. His actual relation to his objects is primitive; they simply must be there when they are needed, and in this respect no consideration can be paid to their interests. The ocnophil's reaction in face of danger contains perhaps the best example of the existence in adult life of what analysis calls 'part-objects'. The demand for the object is so absolute that if the need arises an object, however flimsy or unsuitable, must be there. As the saying goes, a drowning man will catch at a straw.

The philobatic world is quite different. Provided the elements are not hostile, the philobatic pilot is safe—and thrilled—in the sky, the sailor on the high seas, the skier on his slopes, the parachutist dangling in the air, and so on. Danger and fear are, however, provoked by the appearance of independent objects which have to be negotiated since they do not form part of his own equipment. The pilot has to take off or land, the sailor enter or leave harbour, the skier to negotiate rocks, trees or crevasses, the parachutist to jump or land. We may therefore say that the philobatic world consists of friendly expanses dotted with dangerous and unpredictable objects. The philobat lives in the friendly expanses avoiding hazardous contacts with potentially dangerous objects.

Despite these differences the basic structure of the physical world is similar for the two. There is no doubt whatever that roundabouts, parachutes, sailing boats, high-diving boards, etc., are real external objects and that both the philobat and the ocnophil experience them as such. But whereas for the one these present opportunities for pleasure, and are valued and loved, for the other they are sources of torment, to be avoided and hated—or desperately clung to. Moreover, it is well-nigh impossible to convince one that the other world has equal justification, that is, to convince the philobat that firm objects are not necessarily hazardous, or the ocnophil that expanses might be friendly.

Until this point I have deliberately used external physical experiences only, since this made the description of the two worlds easier.

I wish to stress, however, that the same two types exist also with regard to feeling and thinking.

Clinical experience shows that there are people who can feel safe and secure only if they are in close contact with an object or objects. (*Object* is used here in the psychoanalytic sense, which includes people, ideas, sentiments, in fact everything that might be cathected by an instinctual urge.) These people cannot bear to be 'out of touch' with their familiar world of objects and if suddenly confronted with a new idea, a new form of experience, or some uncertain physical or emotional situation, they feel disconcerted and long to return to the safety of their accustomed ways of thinking, feeling and being. They feel lost without their objects, external or internal, and so criticize or exchange them only with difficulty and diffidence. Their objects must be preserved at all costs, since life without them would be unbearable and chaotic. Moreover, these people do not seem to be able to trust their world of objects to look after them; and because they feel that their objects cannot be relied upon to be there for them, they must cling; the only way for them to feel secure is to be 'in constant touch' with their important objects, people, ideas, etc. Objects are merely accepted, not trusted; they are needed but cannot be let out of control.

People of the other type are apparently all out to preserve the feelings of safety and harmony in the mix-up with their friendly expanses even after the emergence of objects into their world. They seem to wish to avoid independent objects and may indeed be found to cherish those objects only over which they feel they have complete and absolute control, which are really part of themselves—of the sort that a sportsman would call his gear or equipment. These people can have objects (which term again includes ideas, people, etc.) which they may leave and rejoin at their whim without any fear that this might lead to recrimination or resentment; in fact it is doubtful whether these objects—the equipment—are felt to have any freedom of their own.

Admittedly the two types described consist

of extremes. One would like to think of an ideal person who, while not abandoning his wish to achieve the one-ness and harmony of his early experience, can still accept objects as friendly and yet independent, who need not deny them their freedom either by clinging or by degrading them to the rank of 'equipment'.

It would be easy to say that clinging to objects, the ocnophilic attitude, is the more primitive of the two, deriving directly from the infant's relationship to his mother. In this case philobatism is to be considered as a reaction formation against clinging, a kind of denial of the need to cling. However, closer examination of the ocnophil's clinging shows that it is anything but primitive. Clinging presupposes the discovery, however dim, of objects that are firm and resistant, with which no friendly mutual penetration is possible and which must be preserved at all cost, as they have an uncanny tendency to disappear, to abandon or drop one. Clinging is therefore both an expression of an anxiety and a desperate attempt to prevent its outbreak. After the traumatic experience that objects, especially the good mother, might leave him, the clinging child—the future ocnophil—accepts the fact that vitally important but inscrutable objects do exist outside himself, but he pretends to himself that they will never leave him if only he can attach himself indissolubly to them.

There is another complication. By clinging one gets further away from the original satisfaction, which was to be held, as one was held in the primal harmony and mix-up by the friendly substances. The inevitable and tragic fact is that the more efficiently one clings, the less one is held.

In contrast, the philobatic world strikes us as fairly advanced. After all, no philobatism is possible without some skill, as instanced by one of our first skills, walking. Skills are based on the acceptance of reality, that is, of the separate and independent existence of objects. Moreover, for the philobat to acquire the necessary skill, he must first learn to see the world 'in true perspective', 'in correct proportions', which means relinquishing the wish to

remain 'in constant touch' with it. These matters point to a possibly later stage of development than ocnophilia.

However, there are a number of highly suggestive primitive features in the philobat's attitude to his 'friendly expanses'. He is apparently convinced that his skill will be sufficient to cope with all hazards and dangers, so that in the end everything will turn out all right. In fact, he feels that he will be able to conquer the elements and that the world will not mind being conquered, will in fact 'click in' with him. It seems therefore that his view of the world is based on a somewhat unrealistic extension of an earlier picture in which there were no separate individual objects, only substances, and on a fond hope that these substances will continue their friendly mix-up with him. A further primitive feature is his attitude towards objects, which he considers either as hazards, or as reliable parts of his equipment; he seems to be confident—perhaps unrealistically overconfident—that his skill will enable him to negotiate all hazards on the one hand, and to pick up any time any object that he may need, on the other.

Thus we have three different pictures of the world, each in a way accessible not only to clinical observation but to some extent perhaps also to psychological experiment. First, there is the world of primary love in which there is no clash of interests between the individual and his environment. As the wishes of the environment are taken as identical it is only the subject who matters, his wishes, needs, desires, have to be attended to and satisfied.

Apart from love and mystical ecstatic experience it is only in poetry, in fiction and in art, that an individual and an important part of his environment, i.e. things external to him, may become one and the same thing. English language has an admirable expression for this exceptional coincidence; it speaks of the 'subject-matter' of a poetic or artistic creation. This is apparently an impossible contradiction, for the subject cannot be a matter, and matter cannot be subjective. Still it beautifully describes the something, which is both inside and

outside the confines of an individual, which is both himself and part of his 'friendly expanses' and out of which something might be created—in fact a mix up.

The discovery that firm and separate independent objects exist destroys this world. From then on, in addition to substances, the existence of objects with their resistant, aggressive and ambivalent qualities must be accepted. Despite many gradations and shades apparently there are two basic ways in which people respond to this traumatic discovery. One is to create an ocnophilic world based on the phantasy that firm objects are reliable and kind, that they will always be there when one needs them and that they will never mind and never resist being used for support. The other is to create the philobatic world which goes back to life prior to the experience that objects emerge and destroy the harmony of the limitless contourless expanses. Objects are felt as dangerous and unpredictable hazards, or as equipment to be picked up or dispensed with. This world is coloured by an unjustified optimism—originating in the earlier world of primary love—which enables the philobat to believe that his skills and his equipment will be sufficient to cope with the elements—the substances—as long as he can avoid hazardous objects.

This train of thought explains why an ambivalent name was given to objects. They are in fact 'objectionable' for the philobat and 'objectives' for the ocnophil. It also explains the fact that languages need two kinds of words for describing the external world, one class of them more distant from the term used to denote ourselves, the subject, and another closely related to it, including one derived from 'mother'. Whereas the first class of words is reserved to describe firm, resistant and sharply contoured parts of the external world with which no mix-up is possible, the latter class is reserved for the limitless substances and the friendly expanses.

These ideas have some importance for certain aspects of psychology. The first is reality testing, which until now has been studied only for the purpose of finding out

whether any statement that something exists outside ourselves is correct or not. This is an important problem but does not cover the whole of reality testing. Obviously the problem of what something existing outside us means to us, whether it should be treated as a danger to our existence or, on the contrary, as something enhancing it, is important enough to merit close study.

Possibly the existence of these two attitudes, ocnophilic and philobatic, can be confirmed by psychological experiments. There might be some differences especially as regard to the senses of touch and sight between the two types; touch being more important in strongly ocnophilic individuals and sight in philobats. Further, I would expect differences in the sense of equilibrium, in, say, exposure to heights or sudden drops. Yet another field in which this train of thought might be of importance, is the theory of the development of the mind; observations of children might produce material to confirm, refute or modify these ideas.

The history of the concepts of the physical world around us gives us an interesting and instructive parallel to the antithesis between the philobatic and ocnophilic worlds. The controversial ideas about the world can be classified into two patterns. One is the atomic conception, according to which everything consists of small, firm and resistant little objects, moving at high speed and knocking against one another constantly. This theory reached its culmination in the classical kinetic theory of the gases and the Rutherford-Bohr model of the atom. The other idea is that of an all-embracing continuum, expressed in the perhaps defunct theory of the all-pervading ether, and the modern theory of the diffuse nature of electrons. The parallel is still more exact, as according to the theory of ether the atoms were considered as discontinuities of the continuous

ether, so to speak holes or whirls in it—clearly a philobatic conception.

It is with some diffidence (for my acquaintance with metaphysics is only casual) that I suggest that these ideas may shed light on the most complex and intricate topic of subject, object, substance, essence, matter, etc. The history of these, and the many related concepts in philosophy, logic and grammar constitutes a real treasure-trove for a philosopher-psychologist. The slight and gross changes in meaning, amounting on occasion to semantic somersault, the unending and apparently never succeeding attempts at clarification starting with Thales and the pre-Socratic philosophers, through Plato, Aristotle, the neo-Platonists, the medieval scholars, Thomas Aquinas, Descartes, Spinoza, Locke, Hume, Kant and after, offer an impressive example of the impossibility of stripping highly emotionally cathected words of their undesirable or unwanted associations.

Last, but not least, these ideas can throw some new light on certain problems of psycho-analytic technique. The present trend is to interpret everything that happens in the analytic situation, also as 'transference', that is, as expressions of an object relationship. The consistent application of this principle has changed our technique considerably and brought handsome profits both in the field of our therapeutic potential and in our theoretical understanding. It is possible, however, that on some occasions we have overshot the target. The present technique means an incessant offering of ourselves to the patient as an object, possibly as an object to cling to. This is equivalent to training him to accept the ocnophilic picture of the world. If my ideas are correct this may not be a suitable solution for everyone and perhaps we may get better results with some patients if we adopt a somewhat different technique based on the idea of the philobatic world.

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NON-SPEAKING CHILDREN: SEVEN YEARS LATER

By LYDIA JACKSON*

In 1950 my paper on 'Non-Speaking Children' was published in this *Journal*. In it I described nine children who were between 3 years 11 months and 6 years 4 months old when they began treatment with me, and whose principal symptom at that time was an almost complete absence of speech. Since the publication of that paper I have maintained contact with three of the children described, and the results of my findings and observations are communicated here.

The children in question were designated in the original paper as Terry (case 1), David (case 2) and Geoffrey (case 9). These boys are now 16 years 2 months; 15 years 10 months and 14 years 5 months respectively.

In this paper the development of two of these children (Terry and David) since 1950 is described, mainly as it was reflected in their use of speech, bearing in mind the close connexion of speech with intellectual and social development, and the profound inhibiting effect certain emotional experiences have on speech, particularly in the early stages. Their other symptoms and behaviour will be mentioned only briefly. I shall describe Geoffrey in more general terms, for my contacts with him during these 7 years were limited to a few interviews and an exchange of letters between his parents and myself.

CASE 1. TERRY

For the relevant facts of Terry's early history I must refer the reader to my earlier paper on this subject. It suffices to say that when he began treatment with me in December 1946, Terry was 5 years 5 months old and had just been certified as mentally defective because of absence of speech and general social unrespon-

siveness. At that time he was mute, incontinent destructive and spiteful towards his two younger sisters. After he had attended the clinic once a week for 4 months, he improved sufficiently to tolerate spending the mornings at a nursery school where he quickly learned clean toilet habits and a few other items, such as washing his hands and hanging up his coat. He began to make a constructive use of play materials and to play with other children. After three terms at school, he knew his letters and figures. At the age of 7 his all-round improvement was so considerable that the Local Educational Authorities deemed him fit to enter the infants' department of an ordinary primary school. On the advice of the clinic staff, however, it was arranged for him to attend a private school with smaller classes, where he began lessons in a group of children some 18 months younger than himself. At the end of his first term there he was described in the school report as 'very obedient, musical; plays with little girls; likes using his hands'. There were no complaints about his behaviour.

The first period of Terry's treatment covered about 20 months during which he had sixty-four interviews. A break of 1 year 8 months followed, during which Terry was given speech therapy, at his parents' request. During that time I gave him eight supervision interviews. In May 1950, at the age of 8 years 10 months, he was re-admitted for treatment, partly on the basis of the speech therapist's report about him, and partly because he was again difficult at home. The school, on the other hand, still had no complaints about him; his end-of-term report contained the remark: 'Difficult to remember he is not a normal boy'!

The second period of treatment included 124 interviews spread over 4 years, until June 1954, when Terry left to go to a boarding school.

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Speech-development

In contrast to his former mutism, Terry was now a non-stop talker, but his speech was quite ungrammatical, the construction of his sentences haphazard, and his enunciation, especially when excited, was very thick and indistinct. He substituted 'ch' for 'sh', left out whole syllables, said 'kukar' for 'sugar', 'lattery' for 'lavatory' and 'cough' for 'cloth'. 'Christmas' in his pronunciation became something like 'Krychm'. He would ask: 'Who did this line chop?' (meaning 'shop', i.e. bought); or 'This is not any more lines?' ('Are there no more lines than this?'). He would make remarks like the following: 'Think I cans make it leg' ('I think I can make a leg for it'); 'People puts his feets down here'; 'What does making noise?'; 'No can make train jump'; 'He's fast 'leep (asleep) when train off lines'. His use of language was more complex than basic English; he spoke rather like a foreigner who had learned some English grammar and then confused all the rules. At times he seemed to be playing with sound deliberately, as when he said: 'Smatch, bang, crotch!' for 'Smash, bang, crash', and grinned as he watched for its effect on me. But the leaving out of articles, conjunctions and prepositions was clearly something he could not help, and he has not entirely mastered them even now. In that early phase, he would sometimes produce a perfect sentence, as when he reproved me for asking him to repeat what he had said: 'I can't go on telling you if you don't *hear* me!' or when he retorted testily after being corrected 'I'll talk as I like!' But the same week, or even during the same interview, he might put a question like this one: 'Do your mans going work?' ('Does your man (husband) go to work?') And as I pointed out that I 'went to work' myself, he replied with an emphatic general statement: 'Ladies doesn't going work, only mans'.

His spelling reflected the confusion of sounds under which he appeared to be labouring. Thus he indicated rooms in his drawing of a house by he indicated rooms in his drawing of a house by writing above the windows: 'Bedron', 'Laber-tree' (lavatory), then another 'Berdrón', now

spelled differently. He covered a sheet of paper with representations of objects in common use and designated them as 'strew' (screw); 'schills' (shoes); 'beedle' (beetle).

The reversal of the regressive process was followed by a long phase during which Terry developed many compulsive, obsessional and phobic symptoms. One of these was the fear of dogs and a refusal to go out of the house by himself, ostensibly because there was a dog in a neighbouring house which barked excessively and jumped up at people. Terry justified his behaviour by saying: 'I don't want dogs to run after me and jump on me. Dogs eats people.' I told him about St Bernard dogs which helped people stranded in the mountains. He retorted stubbornly: 'Don't tell lies. Dogs eats people.' He was thus able to talk of his fears. A few months later, at the age of 9 years 6 months, he made a verbal distinction between the fear of a real and of an imaginary danger. Looking at a picture of a house on fire, he said: 'I don't like the fire burning, I'm afraid I'll get burned', and a week later, explaining his inability to go to sleep for hours after going to bed: 'I'm afraid to dream about a house catching fire. I'm afraid of the dream.'

He was striving to define and to understand words, wanting also to let me know that he understood them, especially when the words had a special emotional significance for him: He asked: 'You know what a thief? He is that steals. They are people who want to be nasty and rude.' On another occasion he announced: 'I know what drowned is. It's when you fall in water and cannot breathe and die.' Some time previously he pushed his sister into the deep end of a swimming bath, and this had seriously upset his mother and had strong emotional repercussions on their relationship.

Once I heard him whisper the word 'beautiful', repeating it several times with a smile. Terry was a good-looking boy, and he could smile in a knowing, secretive way. I pricked up my ears. He asked: 'What does "beautiful" mean?' I did my best to answer this. He continued: 'How do you know what "beautiful" means?' Then, after a pause, he said under his

breath: 'Greeks... they were a long time ago ... they were fond of beautiful things.' Some time later he made this meditative remark: 'I think of you laughing... What makes you laugh? Do you like laughing?' He was 10 years 2 months at the time.

At the age of 10 years 6 months he composed the following 'Nature Puzzles':

It is winter time
It's started (started) snowing
Wind is blowing
Come's a bird
Has a red breast
He flies in snow
Who is he?

ROBIN

Bird with beek
Sharp as could be
He cuts holes in a tree
He build nest in a tree
He eats worms
Who is he?

WOODPECKER

Comes along a bird
Who can dive in stream
And can catch fish
So Bird dive in stream
And caught the fish
And atet (ate it?) the fish for
his lunch
Who is he?

KINGFISHER

In the woods
it's spring time
Birds build ther nests
And lays eggs we only
can take one egg
Magpie pecks the cats
When can to their nest?

His school report contained remarks stating that he was 'working hard, abounding with life and energy, overflowing with interest in every phase of school life, chattering almost ceaselessly and making very dry and apt remarks sometimes, though he easily spoils their effect by repeating them too often! He is

tremendously interested in the words and phrases which happen to catch his fancy.'

He was certainly able to express criticism of other people's work and behaviour in appropriate remarks. Thus, looking at a younger child's drawing of a house, he commented condescendingly: 'The ground standing up and house on top. Children doesn't know.' And when I protested that the buckets of water he wanted me to keep on bringing for his game of 'thunder and rain' were too heavy, he remarked: 'You must be a very weak lady if you cannot carry a bucket of water.'

He resisted interpretation not by mere denial, but by criticizing me. Once, at the age of 11, he said: 'You always say I did it on purpose when I didn't.' And when I professed not to know where something he was looking for was, he told me: 'You are saying this because I'm naughty: you know where it is.'

He had an inquiring mind with a philosophical and a poetic bent, as some of his remarks and his compositions revealed. At the age of 11 he spoke to me about a boy of 8 he knew, who still believed in fairies. 'When I quarrel (argue) with Mark about fairies, he tells me he can see them. Is it his imagination that makes them come out of his eyes and then he thinks he can see them?' And he talked excitedly about seeing 'feathers' of all colours which came into his room when the door was open. They spoke to him in 'gruff voices'; they came when he was afraid they might come. He wanted to know whether 'all children had nasty dreams'. When I told him that I used to dream of ghosts when I was a child, he remarked thoughtfully: 'Feathers are a kind of ghost.'

Some imperfection of speech persisted throughout the 4 years of treatment, but the phobias and compulsions gradually cleared up. Terry was able to take a sudden operation for appendicitis in his stride; he stammered for a week after coming out of hospital, but his behaviour became more integrated and his manner more confident. He told everyone that 'the hospital was not to hurt you but to make you better'. He was always able to dramatize and play out his strong emotional

reactions to news of calamities or accidents of which he read in the papers, such as train or car smashes, or floods. At the age of 12 years 6 months, though badly shaken by an accident when the car his father was driving collided with a lorry, he was able to describe it and to illustrate his description with some toy cars. He said: 'The lorry driver got out and looked very astonished at us. No, I didn't have time to get cross with him, I had a great shock. I was thinking what's happening to me.'

The schooling arrangements unfortunately did not go very smoothly in later years. His private school could not keep him after the age of eleven. His parents chose another private school which accepted him on trial for one term and decided against keeping him. He had private coaching for 7 months and at the age of 12 years 10 months was admitted to a boarding school for educationally subnormal boys where he was very bored and unhappy. He stayed there for a term and a half, and, although he passed through a period of depression, he also profited by its rough-and-tumble, and gained reassurance through finding out that his mother 'did not stop loving him because he fought other boys', as he put it. During the Christmas holidays of 1954 he had his tonsils and adenoids removed, lost a great deal of blood, and when I saw him, was able to speak of this experience in a composed manner, explaining that 'they didn't stitch a vein in my throat properly', and quoting his conversation with the doctor.

He was admitted to a boarding school for educationally backward boys at the age of 13 years 5 months, and made steady progress in integration during the following 2 years. When he came to see me during the holidays, he was usually reluctant to talk about the school, but was eminently fair in his comments on the teachers and the boys. Often he talked of the things he used to do in my room, reminding me of his misdeeds, especially of the occasion when he emptied my bowl of sugar into the water tank: 'I did it because you didn't get cross with me. My mother was very cross with me, and she didn't speak to me for an hour after

that. Why don't you get cross with people? Would you get cross with me if I did it now?'

At the age of 15 years 1 month he drew the plan of his neighbourhood, and indicated, by drawing hearts and arrows, that he was in love with a 'golden-haired girl' whose garden adjoined his.

In his last interview with me, after leaving school, Terry, aged 16 years 2 months, talked to me in an entertaining yet thoughtful way about the changes in the perception of time and place which come with the years. At 15 or 16, he said, time passes much more quickly than when you are a child; the road to school which had seemed to him so long and hilly when he was 8, was now short and almost flat. He then returned to the subject of his former 'naughtiness', and told me with a smile that I ought not to let children do such naughty things as breaking windows or electric lights (he had never broken any), for it 'made them worse'. He also brought up some memories of harsh treatment by his mother which he had never disclosed before.

After a short trial he has been offered apprenticeship at a small factory where his musical and mechanical abilities are being used. His father and he are very good friends. He has achieved relative freedom from an excessive attachment to his mother which held up his emotional development throughout his childhood. Many of his symptoms were reactions to her unconscious hostility and exaggerated fear of *his* hostility and the violent forms it might have taken if it broke out. Now Terry appears to be as 'normal' as a somewhat unusual and original youngster can be, and has a good chance of preserving his balance in the future.

CASE 2. DAVID

For David's early history the earlier paper should be consulted. He was first referred to the clinic at the age of 4 years 11 months. At that age he did not respond to verbal tests, but on the Goodenough drawing test he made a score of 100. His first school refused to keep him because he was 'out of touch', unrespon-

sive to discipline, terrified of other children, or wildly aggressive towards them. At home he developed tantrums and rages, and after his brother's birth, when he was 6, he began to wander from home and could not be trusted in traffic.

He began treatment with me in March 1948 at the age of 6 years 2 months attending once a week. At the age of 6 years 11 months he entered a small private school run on Froebal lines, and was able to remain a pupil there for 1 year and 3 months. At the age of 7 years 3 months he was tested by the clinic psychologist on Koh's Blocks and obtained an I.Q. of 139.

When I reported on his case in 1950, David was 8 years old and had received 73 treatment interviews lasting 45 minutes each. He was then sufficiently integrated to make it possible for him to continue at school and to make progress in his studies. His parents were, however, anxious for him to continue treatment, and after a time it was found necessary to increase his interviews to two a week, later gradually reducing them to one a fortnight, until David went to a boarding school in September 1953, at the age of 11 years 9 months. By that time he had had 152 interviews in all. I saw him also every school holiday (ten more interviews).

Development in the use of speech during treatment

The slow process of integration during the first seventy-three interviews, spread over 21 months, was reflected in David's more coherent and purposive use of speech. At the age of 8 he made, in my hearing, his first general statement. I asked him whether he could skip, to which he replied indignantly: 'Boys don't skip, girls do.' He could, when he chose, adequately explain some of his actions. Thus, as he drew a picture of a boy picking apples and put a crescent moon in the sky above the tree, I asked whether the boy was picking apples at night. David thought for a moment, then said: 'It is just getting light.' He was also more aware of the consequences of his actions and could take verbal precautions against them.

As he found paint going over the edge of the paper on which he was painting and staining my desk, he said: 'Can you give me a piece of newspaper to put under, so that I can stop making a bit of a mess?'

At the age of 8 years 3 months he expressed self-criticism. Seeing another child's drawing of a railway engine on the blackboard he at once drew a small archway in front of it. He stepped back to look at it, laughed and remarked that he had drawn the arch too small. He asked: 'Will the train go through it?' I replied: 'Do you think it will?' David: 'No, because it is too small.' He then rubbed it out and drew another one, just big enough for the engine to go through.

The compulsion to ask questions to which he knew the answer was very much to the fore at that time. He would build precariously, and when the construction toppled over, he would ask insistently: 'Why is it falling?' Or he would push a boat under water and ask: 'Why is it sinking?' When the question was passed back to him, he would usually supply the obvious answer. He reacted 'normally' by surprise to unexpected answers. Thus, when I once replied laconically: 'The law of gravity', David looked up quickly with the words: 'I beg your pardon?'

At the age of 8 years 5 months he began to verbalize self-observation. He repeated under his breath the last words of his own remark, as was his habit, then said aloud: 'It is silly.' I asked 'What is silly?' David: 'Repeating'. (No doubt, he had been told this many a time at home and at school.) He was also expressing verbally resentment at being criticized. Thus, when I compared his behaviour with his young brother's, David protested angrily: 'You must not talk to me about Robin!' And when he objected to my interpretation, he would say: 'Don't tell me!'

Whereas Terry's main difficulty at that age was in enunciating and constructing his sentences correctly, David's chief limitation consisted in an apparent inability to put his thoughts into words at all. Yet the scope of ideas and feelings he was able to express

verbally was steadily increasing, as well as his ability to expand his statements to three or four sentences, instead of an earlier one-word sentence, or a single short phrase usually left unfinished. He was 8 years 11 months old when he remarked musingly: 'It is the 30th to-day. To-morrow is the 1st of December. It is when the concert is going to be. Are you going to be there?' (at his school). A few months later, he remarked, after building a model of a railway station: 'I'm not going to show this to Mum' (who was upstairs), 'because Robin is up there and he will want to touch it. He might spoil it.'

At that age he gave a proper equivalent of the word 'escape' as 'running away'. A few months earlier when I asked him the meaning of some words contained in the captions he was reading aloud, as he looked through a 'Rupert' book, he either replied, characteristically, by a jingle association, or by picking out some element of the picture he was looking at. Thus, to my question 'What are docks?', he replied: 'Rocks'. 'What does it mean—to be curious?' David: 'Knocking at the door.' 'To startle?' David: 'To look round.' 'To lend a hand?' David: 'To turn a handle.' 'Astonished?' David: 'They are looking round.'

Like Terry, he was interested in words, and, at the age of 9 years 11 months, transferring the school situation into the treatment room, he would ask me to play a game of taking turns in writing down names of animals, or Christian names through the alphabet, and proved to be quite competent at it, only occasionally producing something incongruous, such as 'Vessel', as a boy's name beginning with 'V'.

At the age of 10, he was able to give a coherent account of a play produced at his school. He said it was 'about a bad man who turns into a good man because he had some dreams. The name of the man was Scrooge.' He could not, however, describe the dreams, saying that he 'did not remember'.

Throughout the years of treatment David retained some compulsive symptoms. Repeating a few last words of his sentences was one of

these; jumping up and down and flapping or twisting his arms was another. These mannerisms were decreasing slowly in frequency and intensity but became reinforced after his first term at the boarding school. A compulsive interest in trains, railway lines, bridges and tunnels persisted for years. David rarely drew anything else, but while he worked on his drawings, it was evident that he was comparing, making inferences and thinking rapidly and logically. His drawings were always in excellent perspective. Once, as he looked at a picture of a tunnel in a train book, he asked me: 'Is this a long tunnel?' For months afterwards this question remained a ritual, until we both began to treat it as a joke. He would then point at a picture of a bridge and ask roguishly: 'Is this a long tunnel?' I would reply: 'Yes, a very long, very dark one', and he would roar with laughter, afterwards remarking soberly: 'It is actually a bridge, and there is another one further on.' Now and again he used mature qualifying words, such as 'actually', and descriptive idioms, such as 'getting on that way' with spontaneity and appropriateness.

He discovered a disused lavatory at the back of the house and for months insisted on having a look at it every time he came for treatment. When these compulsive preoccupations were linked up by interpretation with his earlier trauma of rectal prolapse and all that it meant to him, David followed it up with much scatological talk, some drawings and verbal exploration of imaginary situations. Some of his questions were much to the point. Thus, when I remarked that food would not stay indefinitely in the tunnel of his bowels (like a train, stuck in a tunnel, of which he was telling me), David asked: 'Why not?' I replied: 'Because your body is made that way, because it works that way.' David: 'What if it works wrong?'

His scatological preoccupations got him into difficulties at school, as he was guileless and inexperienced at concealment. The headmistress did not wish to keep him after he reached the age of 11, and she cut his last term attendance to mornings only. David had been coming to the clinic on his own from the age of 8 years

8 months, and had developed considerable skill in finding his way about. He used his newly acquired leisure to make journeys by underground and 'bus, planning them with maps and almost always getting home in time for tea. His sense of direction and locality was thus developed to an unusual degree, as well as his gift of drawing. He was also improving in his capacity to interpret metaphors. When I remarked that he was 'pulling my leg' and asked whether he knew what I meant, he replied: 'Yes, just for fun'. Asked what was meant by 'a bird's eye view', he replied: 'See from a long way off'.

His increasing capacity for handling words enabled him to compose at the age of 11 years 2 months some rather pedestrian stories about the pictures of my test of Family Attitudes. They had little relation to the pictures and consisted mainly in the description of the outings David himself had had with his parents and in the enumeration of everyday activities, such as having meals and going to bed. On the other hand, he showed a certain fastidiousness in his choice of words, and, in dictating a story, first told me to write 'going round and round' (about steps of a staircase), then changed it to 'twisting' and finally to 'winding'. He would roll some words round his tongue, and as he was repeating the word 'puzzle', I remarked on its strangeness. He smiled confirmation and murmured: 'The puzzle puzzles me.'

Nevertheless, he remained ingenuous and without much idea of social conventions. Thus, at the first meeting of the family with his prospective headmaster, at which I was present, David interrupted the exchange of greetings between the parties by asking the headmaster: 'I say, do you know where the "gents" is?'

At the age of 11 years 9 months David was admitted to a boarding school for educationally retarded boys. The clinic team felt very doubtful about this move, but no suitable day school could be found in the neighbourhood. After one term away David had noticeably regressed. Most of his mannerisms became intensified, he could sustain no conversation, and his mother was unhappy, finding him

'worse than he was before he went'. His school report, however, was laudatory; the headmaster's remarks contained a phrase: 'Had never met a better-natured boy'.

During subsequent holiday interviews no steady improvement was evident: sometimes David behaved in a more integrated way, sometimes he showed signs of increasing withdrawal and splitting. The compulsive jumping up and down of the earlier years gave way to hand-rubbing; he giggled without apparent reason and made hissing noises with his lips. In his better phases he drew or asked to play 'a mixed up words game'. In both these activities he was conspicuously better than he had been. The drawing for once was not of railway lines, but of a suburban street with a small van standing in front of a house. David said about it: 'It isn't good. It's like a little boy drawing', but few little boys could have drawn these objects so accurately. He gave me unsolicited help with mixed-up words by saying that *oyednk* was something 'you sometimes see at the sea-side, on the sands' (a donkey), and *ptoihpm-supao* was 'bigger than an elephant, and sometimes it gives a big yawn' (hippopotamus).

At the age of 13, his fondness for puns made him remark, as he was looking at a map: 'Washington—they do washing there. Bagdad—they make bags there!'

Compulsive questions gave way to a compulsive exploration of a series of suppositions, usually of a strongly sado-masochistic content, which David clearly enjoyed putting forward even when he was horrified by them. Thus, after a term during which he had been taken ill in the school chapel, he pressed me to reply to these questions: 'Suppose there was a chapel as small as this table and they used a lot of incense, how strong will it smell then? Will it smell different in different parts of the country? If the ceremony lasted all day, how strong would it smell? What would happen if one ate incense?' And on another occasion: 'What would happen if a blind man stood in the middle of the road? Would he die? What would happen if a blind man drove a car?'

At the age of 13 years 9 months, he had a

particularly disturbed phase. His physical health was not too good. He was growing fast, but not gaining much weight, had nasal catarrh and frequent bouts of diarrhoea. In the summer holidays he saw a cow being milked, and this had a deeply upsetting effect on him. In his interview with me he looked through a boxful of lead animals, picked out a cow, handled it tenderly and pressed it to his cheek. Then he pointed at its teats and said: 'Sweet little things.' I made some remarks about human mothers feeding their babies with their milk. David said nothing but looked sad. Some minutes later he told me: 'You mustn't talk to me like that: it hurts my feelings.' He confirmed that he did not like the implied comparison between a cow and his mother. This was followed by a series of questions: 'Where does the milk come from? These teats that my mother has, has she got any milk now? What happens to it afterwards? What is it like? What colour is it? What does it taste like? What would happen if you gave mother's milk to a calf?' Then, with a kind of horrified excitement: 'When a cow gets old and stops giving milk, you know, when they take her to the butcher's for meat, what do they do with the udders? What would happen if you took a blow-lamp and held it under the cow's udder? What would happen to the milk? What would the cow do?' And finally: 'You wouldn't tell mummy what I've told you about the teats, would you?'

His school reports, however, remained consistently good: he was 'working hard', 'making a good effort'; his reading and spelling were 'excellent'; in his last year he even 'made some progress' in history and religious knowledge, which at first appeared 'quite beyond him'. He never learned to take part in games or other group activities, such as acting.

Since then, David maintained steadier progress; when I saw him a few months ago before he returned to school for his last term there, he looked and conversed like a 'normal' youth, showing none of his former mannerisms. When I asked him whether he now felt that it was a good thing, on the whole, to have gone to

a boarding school, he replied: 'In part... but most of the time I really hated it...'. He repeated under his breath 'hated it'... (When I asked Terry the same question, he replied: 'Yes, because I learned discipline and to be with other people.')

David talked on, telling me about the walks he had had during the holidays, described the weather, drew the plan of the locality, mentioned a certain 'disused railway' and a tunnel! He said that on previous holidays they had gone through it, but this time 'there was an awful smell, the cows had been there, it was dark... Robin didn't like it, nor did I. We did not go in. There was an awful smell... you get diseases...' In these few sentences most of elements of his former conflict appeared to be brought together: feeding—evacuation—danger of destruction or damage, and the fear and attraction of the objects symbolizing them. It seemed, however, that for the time being David had come to terms with them. What happens later would depend largely on the degree of protection from the hazards of life he would obtain in the future. He would have to continue living in a protected environment.

CASE 9. GEOFFREY

Geoffrey began treatment at the age of 5 years 8 months when he was mute, manneristic and incontinent. In the subsequent 11 months he attended thirty-nine times, each interview lasting 45 minutes. The process of regression was arrested and to some extent reversed; Geoffrey was beginning to relate himself to people. Incontinence ceased and some constructive play, drawing and spontaneous use of speech began to make their reappearance.

At this stage his parents left the neighbourhood and no treatment for Geoffrey could be obtained where they lived. I remained in touch with the parents by correspondence, and Geoffrey's mother brought him to see me three times during the last 7 years. The first visit took place 1 year 3 months after the termination of treatment. Geoffrey gave me some flowers his mother had brought and repeated the words of greeting she had taught him, as he looked up

steadily into my face. He came to my room, went at once to the water tank, handled a rubber duck which could not float upright and said: 'It keeps falling over.' He then became entirely absorbed in sand and water play, just as he used to be, and sang and shouted in great excitement. As formerly, he tried to drink sandy water from a rusty tin; when I replaced this with a glass of fresh water, he would not drink it. As I dried his hands before taking him back, he leaned against me. I asked him whether he remembered who I was. He answered: 'Yes', at once and gave my name. When, however, I asked him how old he was, he replied: 'Three'. He was, in fact, nearly eight. His mother told me that he made very little progress in learning to read or write, tolerated no contact with other children, 'just ignored them', and had no awareness of common dangers.

Seven months later Geoffrey was again brought to see me. After presenting me with a bunch of flowers, he marched into my room before I had invited him. His mother and I followed, and while we talked, Geoffrey wandered around the room doing some of the things he used to do, such as pouring paint water into the sand, or using a wet brush to go over toys as if painting them. After his mother had gone out of the room, he began to dance and sing, with rigid, ritualistic movements. I asked him whether he would like to draw. He said: 'Yes', went to the blackboard and drew some wavy lines with a green chalk, saying: 'Must draw some water'. Then, very rapidly he drew a boat with a mast and sail and wrote the letter 'A' on the sail. I asked: 'Are there any men on the boat?' He replied: 'Yes, I'm going to draw the men', and drew two little figures with blue chalk. As he added a large object at the back of the boat, I asked whether that was a rudder. He repeated, 'A rudder' several times. He continued by drawing a house and telling me what he drew: 'the green curtain, the path, a palm tree, a gate'. I asked 'Are you going to draw a little girl?' He drew a human figure, using a brick-red chalk, went over it with a green chalk, and said 'her clothes'. He added a

blue top hat to her head, saying, 'a boy's hat'. He then drew parts of the face naming them: 'the nose, the eyes, the ears', then added in front of it a huge pipe with a cloud of smoke coming from it. The figure thus became a mixture of girl, boy and man. Throughout the interview Geoffrey echoed his own remarks and mine, repeating them either in full or only the last word or two.

His mother reported some progress in that Geoffrey now sometimes answered questions, although he never asked any. He did, however, ask for things he wanted, instead of taking them as he used to. At the little village school he was attending, he learned to read and write a little, but made no progress in arithmetic. He soiled himself only when he was unwell.

His father told me that Geoffrey was now showing more affection for his parents than he had done hitherto. Both parents were wondering whether it would be damaging to part with him at this stage, yet both felt that to go on as before was too much of a strain. They wondered about the effect Geoffrey's behaviour might have on their younger child, a girl. Geoffrey was 'thieving', i.e. taking food and drink secretly from the larder; he played with fire and would not be deterred by the demonstration of its dangers his father had given him and he remained unaware of the risks he ran by wandering out into the road.

Attempts were made during the next 18 months to secure a place for Geoffrey in a mental hospital for observation or treatment. They failed, and the parents then asked for help in placing Geoffrey at a special type of boarding school. This was done in stages. At first, Geoffrey and his mother stayed in lodgings near the school, and Geoffrey attended as a day pupil for one term. After that his mother returned to her family, while Geoffrey became a boarder. He refused food and regressed in other ways, but after a time seemed to accept the separation. He remained at that school for 4 years, coming home for the usual holidays. The mother saw no improvement in him and regretted the lack of formal instruction. When Geoffrey was 14 the school informed his parents

that they could do nothing more for him, and he was brought home.

Geoffrey came to see me a few weeks later, in May 1957. A slender neatly built adolescent, he looked frightened, frowned and grinned alternately; his face twitched. While I talked to his mother, he wandered about the room, touching things, or performing some of his compulsive movements—rigid gestures with arms and hands, slight hopping, putting his hands behind his neck, etc. At one point he suddenly went down on all fours and peered at my legs, his face very close to the floor.

His mother told me that he had 'forgotten all she had taught him before he went away'. She was now again teaching him to read. As she urged Geoffrey to tell me what the story they were reading was about, he barked out unintelligible replies in a hoarse voice and laughed. I asked him how old he was. He replied promptly 'Five'. His mother reproved him, so he continued hurriedly 'Six, seven, eight...' until, with her prompting, he finally got to 'Fourteen'. His mother repeated, displeased: 'Well, how old are you then?' He said 'Four...' then, as she voiced her dissatisfaction, he added hastily 'teen'.

At first he did not want to follow me into another room, but when the invitation was repeated half an hour later, he agreed to come. He showed pleasure and animation when he saw toys strewn over the room. He picked some up, put boats into the water tank, moved them about and commented on what was happening. His voice was now clear, his enunciation precise. He said 'it sank to the bottom'. Holding up one lead toy after another, he remarked 'A soldier marching', 'A soldier using his gun', 'A farmer', 'A rhinoceros', 'A giraffe with brown spots', 'A goat' (all correctly). When I asked him questions, he answered with 'yes' or 'no' appropriately. 'Were there any girls at your school?' 'No.' 'Does your father talk to you much?' 'No.'

I offered him some painting materials. He declined at first, but later, as he held a toy lorry in his hand, he said suddenly 'Copy the lorry'. I placed a sheet of paper before him. He tipped

a brush into a jar of thick yellow paint and outlined an aeroplane, saying: 'A plane with yellow wings.' It was a realistic attempt at first, for he tried to paint red-and-blue circles on the wings. But as he thrust the brush into thick paint and piled it on to the paper, Geoffrey's excitement increased. The objects he painted and named became less and less recognizable. 'A boat' was a reddish-brown blob, a square red patch was 'a sail', two straight strokes 'green masts', another brown blob—'a cabin'. In the opposite corner of the sheet he painted a long brown streak—'a path'—spread some green paint on both sides of it—'grass'—put green spots over brown—'grass' again. In between, he rinsed the brush vigorously in a jar of water, then put it into his mouth, staining his lips with paint.

He was most reluctant to stop this activity and to come downstairs, but after he had been with me nearly an hour, I persuaded him to come along. He said 'Wash my hands', and came to the bathroom where he washed them carefully. After joining his mother, he sat down, leaning his head against the back of the chair and staring in front of him. All animation and excitement left him.

Thus, at the age of 14 Geoffrey was no better able to initiate or sustain conversation than he was at the age of eight. His enunciation of words was precise and clear as it had been in childhood. According to his mother, he had lost the little formal knowledge he had acquired before he went to boarding school. He retained all the hebephrenic mannerisms he had at the age of 6, and he seemed as inaccessible to ordinary human intercourse as he was in the early phases of his illness.

CONCLUSIONS

It is not possible in a paper of this length to examine fully the many questions which arise in connexion with the treatment of such children. It seems clear, however, that the reversal of the process of regression in such cases is only the beginning, and that a great deal remains to be done in subsequent years. Yet in my opinion treatment is well worth

while, despite the time and labour involved, even if the financial burden on the community of keeping such patients permanently in institutions were the only, or the most important consideration. Psychotherapy, even if it takes years, is still cheaper than institutional care. The saving in human distress is, on the other hand, inestimable.

In undertaking treatment, however, it may be wise not to set oneself too high an aim. Family histories of children who fall victim to this type of illness, suggest the presence of an inherent susceptibility, or a genetic flaw of varying gravity, which may baulk the most thorough and prolonged efforts at rehabilitation. One can rarely hope for an essential change in the personality of the patient, however young. Yet, by helping parents, family doctors and health visitors to a greater awareness of the significance of certain kinds of behaviour in infants, it might be possible to avoid subjecting such infants to particularly dangerous stresses and to prevent some of the early psychotic breakdowns.

The prognosis appears to be more favourable in cases of child psychosis designated as 'sympiotic', and less so in cases designated as 'autistic' by Margaret Mahler (1952). Terry and David both belonged to the first type of case, while Geoffrey was more 'autistic'. Both Terry and David were of an affectionate disposition. Terry's demonstrations of affection had real spontaneity and warmth. He developed an attachment to every person who took an interest in him, with the exception of one or two rather stern teachers. David had 'crushes' on women teachers and little girls at school; he 'ogled' them at school concerts, to his mother's discomfiture. Geoffrey, on the other hand, has always behaved like a little automaton; his relation to his mother appeared to be rather like that of a puppet to his puppet master. In contrast to the other two, he had never shown any warmth towards me.

The personalities of the parents must be considered in this connexion. Their capacity for co-operating in treatment is of the utmost importance in such cases. The parents of Terry

and of David were warm-hearted people who did their best to adapt their handling of the children in the light of deeper understanding acquired during the years of regular contacts with the clinic team. They had their own problems to solve, and the degree of tolerance, insight and detachment required from them in the circumstances could not always be maintained at the same high level. Yet they went through with it, and emerged at the other end more at peace with themselves and the world than they had ever been.

Geoffrey's parents, unfortunately, were less able to view his illness as principally a breakdown of personal relationships, and to accept their part in restoring them. By deciding to leave the neighbourhood of the clinic at a critical stage in his treatment, they showed that they valued better material conditions more than Geoffrey's mental health. It is therefore clearly desirable to make as certain as possible of the parents' capacity for co-operation and their ability to sustain treatment before embarking upon it.

Important, too, is the question of schooling of such children. Ordinary schools, as a rule, cannot hold them. Both Terry and David had to go to small private schools, and Geoffrey had to be withdrawn from his, until the family moved into the country where he attended the village school at which he learned hardly anything. Terry and David did learn, but they had to work in a group of children about 18 months younger. At their boarding school for educationally retarded boys they were subjected to formal teaching in the usual subjects. Some of these they were able to master. Both boys were above average in the mechanics of reading and writing. Terry was 'surprisingly good' at nature study, David at learning French. Neither could master arithmetic beyond simple calculation. Religious knowledge and history were 'rather beyond them'. Yet both these boys had very considerable special abilities. David could draw better than any boy at the school, winning the Art prize every end of term. Terry was most competent in dealing with things. He could mend, take to pieces and

assemble quite complicated mechanisms. He was also exceptionally musical, played the piano by ear, and was said to have 'absolute pitch'. It seems that a great deal of time and energy had been expended on teaching these boys mental skills they could not learn and were not likely to apply. This was done partly to meet the parents' anxious wish that their children should have the training accorded to 'normal' children and be taught everything the others were taught. But if there existed a school where teaching were better adapted to these boys' special gifts and limitations, such teaching would have been more productive,

more constructive and liberating than the traditional kind, which forced them to sit through hours of lessons during which they were said to be 'in a dream'. A small day school for children of this type would be more economical than supporting them in mental hospitals or schools for maladjusted.

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SOME DISTURBANCES OF FAMILY FUNCTIONING ASSOCIATED WITH PSYCHIATRIC ILLNESS*

By PAULINE MORRIS

This paper is an attempt to present a descriptive analysis of the social effects of mental illness on the families of a sample of twenty male patients drawn consecutively from the referral register of the Social Rehabilitation Unit at Belmont Hospital in the spring and summer of 1956 and to indicate some characteristics of the treatment process as it has affected the family. It will discuss ways in which the character and quality of the family relationships are affected by the illness, including the patient himself as a family member; and those in which a treatment programme might be adapted to deal with problems which arise for the family as a social unit, both as a result of the illness and of treatment.

The pattern of social relationships within any family is a dynamic one, and subtle changes may be continually occurring, especially in what may be described as the 'emotional toning' of relationships. These are not the primary concern of this paper, although they may be referred to *en passant*. On the contrary, in order to simplify the discussion, the data are primarily examined from a static viewpoint in time, namely the moment of initial contact with the family and the patient's entry into the Unit.

The material used has been extracted from a larger study of which it forms part, a study involving contact with the family in three distinct phases: (i) after referral but prior to admission; (ii) during hospitalization; (iii)

* The 'Family Study' is part of a larger research project for the Social Rehabilitation Unit at Belmont Hospital, Director Dr Maxwell Jones. The study was supported by the Nuffield Foundation, and directed by Dr R. N. Rapoport. An over-all orientation of the Family Study has been published by Rapoport & Rosow (1957). Manuscript received 13 September 1957.

after discharge for a period of 6 months or more where possible. Of the twenty patients, fourteen entered the Unit for varying periods. Six were interviewed prior to admission but failed to come for treatment. Of the fourteen patients who entered the Unit, seven were married; of those who failed to appear for admission, four men were single, one was married and one had recently begun co-habiting.

Table 1. *Composition of sample*

	Married	Unmarried	Total
Treated	7	7	14
Untreated	1	5*	6

* One of these was co-habiting.

DEFINITIONS

The term 'family' here refers to the nuclear family and in the case of married men, information about siblings or parents is not included as there is insufficient data. For the purpose of the analysis the family is regarded as a structured group, each of whose members has a social role, the nature and limits of which are determined in terms of a structure of action which is normative in a cultural (though not necessarily a statistical) sense. The rationale or objectives of the family group may be regarded as determined in part by biological and in part by cultural factors, and without digressing to consider their precise nature, in an urban industrial society, the efficient family is normally expected to provide:

(a) A structured economic setting wherein, through the division of labour, the material needs of its members may be satisfied.

(b) For the legitimate satisfaction of the sexual needs of the parental couple.

(c) An approved setting for the procreation and socialization of children.

(d) A degree of emotional security and stability for its members.

(e) To operate as an informal agency of social control both in the extra and intra familial activities of its members.

The achievement of these goals is primarily dependent upon the adequate performance of each individual concerned in his or her social role; conversely, inadequate performance by any one individual will not only prevent their attainment by the family as a whole, but, by disturbing so to speak what is a culturally determined 'division of labour' will impose additional demands upon other family members and so impair the role performance of others.

'Mental illness' is a term which, although its meaning might be thought self-evident, does in fact need some clarification. Psychiatry makes a fairly broad distinction between the neuroses on the one hand and the psychoses on the other. Apart from the special symptomatology which distinguishes specific types of disorder, mental illness may be generally recognized by a disturbance of the emotional balance of the individual, or a distortion of his perception of the reality situation, or both, either of which may be accompanied by a variety of psychosomatic symptoms. Normally the psychoses involve such a degree of confusion or distortion in the process of perception and remembering, that the individual is at times divorced from reality; where the neuroses are concerned, however, the individual's problems are focused upon issues which have a basis in reality in that they have an existence beyond the confines of his mind, and a meaning for others. Most neurotic individuals remain in the community, often untreated, for a long time after the first appearance of neurotic symptoms.

All the patients discussed in this paper may be said to suffer from personality disorders and they present problems of social adjustment, i.e. chronic unemployment, alcoholism, anti-social activities, etc. All these are accompanied by a disturbance in interpersonal relationships, though not necessarily in every social setting.

So far we have considered a definition of

mental illness based upon clinical and sociological concepts, but patients and their families also have 'definitions' of mental illness, although of course they are seldom explicit and only discernible in an oblique way. It is in fact probably truer to say that for those patients who may be clinically described as neurotic, the families' concept frequently excludes the term 'mental', but is much more an idea of illness which, though perhaps somatic in its symptoms, clearly has other characteristics related to the patient's state of mind in a way in which ordinary physical symptoms are not. The families' own definitions may be observed by attempting to identify that point in time at which the patient is recognized as 'ill' rather than merely perverse or difficult. In a recent article, Mrs Wootton (1956) has suggested that the distinction between neurotic behaviour to which blame is not attached, and wilful or socially irresponsible behaviour is much less clear than is often supposed. Certainly in a number of cases in the study, some family members persisted in making adverse moral judgements about the patient, refusing to recognize the existence of any kind of 'illness'.

EFFECTS*

From our sample of twenty, of those admitted to the Unit the problem of mental illness is known to be a lifelong one in ten cases out of fourteen; in the remaining four it is certainly of many years standing. Of the six who did not enter the Unit, it is so in four cases, though there is insufficient information relating to the other two. As a result, the broad effects of the illness upon the functioning of the family are likely to be gradual rather than dramatic, a factor which may have an important limiting effect on treatment goals. It is, therefore, often difficult to distinguish between behaviour which is pathological or non-rational, and an impediment to the effective functioning of the

* One of the major difficulties in the analysis of this type of data has been usefully to distinguish between 'cause' and 'effect' and between 'effects' as distinct from 'presenting symptoms'.

family, and behaviour which, though abnormal in a cultural sense, represents nevertheless a reasonable response to the functional requirements of a situation. It is suggested that the personality structure of the patient may have resulted in a 'natural' patterning of relationships as distinct from a situation in which a 'normal' pattern may have been disturbed by illness. For example, a marriage may be based on the pathological needs of both partners and what may appear as a relationship characterized by aggression and therefore doomed to failure, may, by virtue of the fact that both partner's neurotic needs are met, continue to flourish. The extremely important ramifications of this problem are discussed by contributors in a recent book (Eisenstein, 1957). On the other hand, a marriage based on more normal ways of relating may subsequently break up due to the illness of one partner. It may, then, be only possible to observe the effects of the illness in terms of the discrepancy between the *actual situation* (the concrete pattern of behaviour) and the *ideal desired*, or alternatively the state of affairs which the family may allege existed before the onset of the patient's illness. One might note that the actual behaviour may be as much determined by unconscious needs as by reference to a set of ideal norms.

Bearing these facts in mind, it is possible to examine the families in the sample in terms of their performance of the functions enumerated earlier in this paper:

(a) *The provision of a structured economic setting*

Broadly speaking, where male patients are concerned, the family is prepared to go to considerable lengths in tolerating what is essentially deviant behaviour, but it is usually when he fails in his economic role that such tolerance is reduced, or it becomes easier to define or recognize the patient as 'ill'. Not only does he fail to provide money for the home, but at the same time his continuous presence there increases the possibility of friction and conflict. All the while the man is able to support the

family he is at least fulfilling one set of requirements, although he may have grave difficulties in his interpersonal relations. When, however, he cannot even do this, it usually becomes apparent that something is really seriously the matter. Analysis of this problem among patients in the sample is shown in Table 2. We see, therefore, that the incidence of work failure is extremely high: eleven out of fourteen admissions. With regard to the remaining three, it is perhaps significant that work was the sole area where the patient *could* perform adequately and recognition of this fact by the family was instrumental in drawing attention to the existence of a problem in another sphere which could be defined as 'illness'. Of those not admitted to the Unit, the three who were successful in work situations expressed themselves as reluctant to give up work or jeopardize work future by coming into the Unit. Thus whilst the patient and his family were able to define the problem as one of illness and to consider its treatment by hospitalization, they were not prepared to take a step which would temporarily unbalance the domestic economy. This is an important consideration because it suggests a discrepancy between the view of the community (as represented by the referring agency) regarding the patient's need for treatment and his own or that of his family.

Table 2. *Major presenting problem*

Cases	Work failure	Other disorder	Total
Treated	11	3	14
Untreated	3	3	6

In this context the family may be said to provide a form of economic organization which ensures the material survival of its members in a competitive world. One aspect of the role of the married man is clearly defined as that of principal source of money income, though in the case of unmarried adult sons living at home, their contribution may be of almost equal financial, if not cultural or emotional importance. The son and the father alike make

common use of the capital equipment of the home and expect to receive food and physical comforts in return for their financial contributions. In this setting, perhaps more clearly than any other, the role failure of a given member may completely disrupt the balance of the system within which a division of labour is strongly buttressed by sentiment.

While observing the high incidence of work failure, it is perhaps surprising to find that financial difficulties in themselves are largely significant in terms of the emotional connotation of money, rather than in terms of actual poverty. In only one case was the family placed in a position of secondary poverty, and economic failure became of vital importance in its own right. There is probably a close relationship between the virtual absence of poverty among families, and the provisions of the social services: these effectively prevent destitution, but the significance of being a recipient rather than a contributor in a society which places so much emphasis on material objects as goals of achievement, cannot be underestimated. Both husband and his family may feel a degree of guilt and shame at being in this position, although of course they are not always conscious of such feelings. Where unmarried men are concerned, their failure to earn, whilst not necessarily jeopardizing the family economy, puts them in an invidious position and one which frequently invites hostility from other wage-earning members.

In those cases where work failure is the point at which the definition of illness becomes operative, the situation becomes encrusted with secondary features which themselves become important reasons for which admission to the Unit is sought. In our sample four cases of work failure were associated with hypochondriasis and psychosomatic symptoms. The resultant confusion between primary and secondary problems is of particular importance to the family who often find it easier to accept and deal with the secondary problem than to seek the root of the difficulties—much of their behaviour is rationalized in this way. Amongst treated patients in seven instances (three

married, four unmarried) shortage of money was cited as a reason for wife or mother going out to work, but in three of these cases (all unmarried) the mothers themselves attached the greatest importance to money and the possession of material goods, and whilst deploring the financial strain put upon them by sons unable to earn, they went out of their way to keep these sons provided with money and food. Thus, whilst they saw their work as an 'effect' of the patient's illness (to make up the financial loss), it is most likely that their need to work was part of the same psycho-pathology which caused them to over-protect their sons, and in this sense they may have contributed to the sons' illness. In the case of the married men, all three wives who cited their husband's economic failure as a reason for working, in fact continued to work even though their husbands obtained jobs; so it becomes obvious that their need to work is connected with other aspects of the relationship than the economic one alone.

In four cases financial strain was given as a reason for restricted social activity, but in no case was it a primary cause, only a contributory factor, the patient in each instance suffering from psychosomatic symptoms which prevented any form of social activity—including work.

A further secondary feature of economic failure in married patients, is that of deteriorating marital relationships and the withdrawal of wives from their husbands. A more explicit discussion of this appears in a later section of this paper, that dealing with sexual needs.

In five treated cases (two married, three unmarried) financial difficulties appeared to play an insignificant part in the total family situation. Three of these appear in Table 2 as having no work failure, hospitalization being due to marital disharmony or social isolation. It seems that the existence of 'secondary' symptoms may be much less where there is no work problem. The other two were unmarried men living in a home where the father was earning well, as were other siblings, and they were prepared to make considerable allowances for the

sick member of the family. In one such instance, the illness was of lifelong duration and the family's expectations, in terms of earning ability, were minimal.

In terms of the effects of the illness on the families in the field of work problems, a distinction should be made between skilled and unskilled workers. In the total sample, twelve out of twenty patients were completely unskilled, the remainder semi-skilled or skilled operatives. In the case of skilled operatives four were treated and four untreated, in the case of unskilled workers ten were treated and two untreated. Furthermore, of the total of eight skilled or semi-skilled only four were work failures and three of these received treatment; in the group of unskilled all but one were work failures. For the families of unskilled workers the effect of unemployment or underemployment is likely to be considerably less upsetting: the culture is geared to accept casual labour, dock-work, road mending, etc., and it is expected that there will be times when the man is not working—it is not seen as idleness. This is quite contrary to the expectations of the skilled group and their families; there is a tradition of relative security in the labour market, and if the men are idle it is because of some specific reason concerned with themselves and their own inadequacy. It is relevant here to make a distinction between types of work failure: it may be a total inability to work, or it may be an exceedingly high job turnover, and if the latter, the preceding discussion will explain why this is so much more readily acceptable amongst unskilled workers and their families.

Among the patients who entered the Unit, only two had families which could be said to provide the security of a structured economic setting. The importance of this would seem to be that if the family is failing to function efficiently in a context in which the roles of the family members are defined by culture perhaps more clearly than anywhere else, then the deterioration of interpersonal relationships within the family will inevitably be accelerated.

(b) Provision for the legitimate satisfaction of the sexual needs of the parental couple

With one possible exception, in every family with a member involved in treatment, the sexual relationship between the patient's parents was most unsatisfactory. In considering the effects of psychiatric illness upon this area of behaviour, it is necessary to distinguish between the families in which the patient was a husband and/or father, and those in which he was a son.

(i) Married patients

In two instances the referral of married men had been precisely for difficulties centering around marital disharmony: here the absence of a satisfactory sexual relationship was essentially a symptom of the problem rather than an effect of it. In the case of the five remaining patients who were treated, the illness contributed to the destruction of the relationship. In the previous section it has been suggested that a secondary feature of economic failure is that of deteriorating marital relationships, and it could be observed that the withdrawal of wives from their husbands was not unrelated to the husband's failure in the work role. Bearing in mind that the majority of patients were of working-class origin, it is quite consistent that such withdrawal should take place, when the subcultural tradition defines the sexual relationship in terms of reciprocity—'good husbands keep their wives supplied with money and good wives give their husbands sexual satisfaction at regular intervals'. Just as wives may feel resentment against husbands who have not kept their part of the bargain but expect them to keep theirs, so too, husbands may feel guilt about making demands to which they do not feel entitled. Observation of these married couples in both interview and treatment situations, indicated that the frustrations and tensions generated by sexual maladjustment found their expression in most other spheres of interpersonal relationships within the family. We have no way of estimating how frequently these maladjusted people

sought satisfaction in extra-marital activity, but the frequency is probably negligible. Because the mechanism of denial was apparently used so extensively, sexual feelings tended to be repressed and transposed into other fields, to emerge in such ways as continual nagging and argument.

(ii) *Unmarried patients*

The position of the unmarried sons is particularly relevant in this context of denial and repression in that, although our over-all culture tends to ignore the fact that unmarried persons have sexual drives, the failure of parents to enjoy sexual harmony is not without its effects upon their children. In the majority of instances these young men were unable to enter into relationships with women of their own age and both they and their parents found difficulty in visualizing marriage for them in the future. Strictly speaking, however, this cannot be considered as an 'effect' of illness, where the illness was that of an unmarried son.

On the other hand, the effect of a son's illness, quite apart from any symptoms taking the form of deviant sexual behaviour on his part, was very considerable upon the marital relationship of his parents. Each of the unmarried men who entered the Unit may be described as a 'dependent son', that is to say his emotional ties with the mother were such that his independence of action was often very restricted and his feelings centred about his mother within the family circle. In turn the dependent sons restricted the activities of their mothers (as will be seen later when social isolation is discussed), and they exacerbated difficulties in the parental relationship. Where this happened the son became a convenient focus for hostility on the part of the father or, in three cases, of both parents. Among the seven sons admitted to the Unit, in two cases the parents directly blamed the patient for their own domestic disharmony; in these instances the marital problem had been in existence for a very long time, and while difficult to trace the history of discord from the available material, it would seem that whilst the son's problem did

not initially cause parental friction, it aggravated it. Of the four dependent sons interviewed but who did not enter the Unit, this appears to be true in at least one case: in the other three, although it is not clear whether the parents do in fact blame their sons, marital disharmony undoubtedly exists and is accompanied by the mother turning all her affection on to the son.

Unfortunately treatment of the dependent sons in the sample did not result in their achieving a sufficient degree of independence for it to make changes in the sexual relationship of the parents.

(c) *An approved setting for the procreation and socialization of children*

In all our families, the children were, so far as is known, born in wedlock, but for this discussion socialization is perhaps the more pertinent issue. With one exception (and a possible second), none of the families involved in treatment were able to provide a satisfactory setting for the upbringing of children.

(i) *Married men*

In our sample two families had no children: in one instance this was a second marriage and the husband 'forced' his wife to have an abortion. This appeared to be a situation in which a younger man was married to an older woman and where the relationship was very much like that of a mother and her son. The birth of a child would undoubtedly have created new problems with which both would have had difficulty in dealing. In the second case where there were no children, one child had died at 4 months and there had been one miscarriage; the couple themselves connect their childless state with the fact that the husband's elderly parents have been living with them for the past 18 years. The remaining five cases treated had families of one, two or three children, but in all of them the sexual relationship of the parents was a major problem, the wives of three being frigid (one man blamed his impotence on to his wife's frigidity), and in the fourth instance the

problem was denied by adopting the norm that sexual intercourse plays no part in the marital relationship apart from child bearing. We can only surmise the problems facing the development of the children of these patients, but it is clear from the tendency of the wives to turn to the children and to see their husbands as inadequate fathers, that such children lack a good male figure with whom to identify. In the one family where one of the children is grown up and at work, his role in the family has very much replaced that of father (i.e. economically and as companion to mother and younger sibling). The father in treatment came to recognize some of the jealousy he felt for his sons, dating back to earliest childhood, and although we know insufficient about this in the lives of the children, it is certainly an 'effect' of father's illness. One family where two children are recognized as anti-social, will be discussed later in this paper under section (e).

(ii) *Dependent sons*

When the patients are dependent sons living at home it has already been suggested that they find considerable difficulty in relating satisfactorily to young women and, what seems equally significant, although the parents express a desire for their sons to marry and be independent, so often they are incapable of visualizing such a state of affairs. The marital disharmony of the parents is accompanied by distorted perceptions of the concept of marriage and sexual values generally among the unmarried sons, and the chances are small of his assimilating a system of behavioural patterns acceptable to the world at large, due to the fact that the models from which he would naturally learn were themselves deviant, not only from the norms set by society, but often from the personal norms of the parents themselves. At first sight the inability to make satisfactory social relationships in later life appears as an 'effect' of illness; it is likely, however, that a similar situation in the lives of the parents was a contributing factor to the son's illness. Not only were the patients in the sample unable to make good heterosexual relation-

ships, but in four cases they displayed strong, if covert, homosexual tendencies.

Overprotection of the dependent sons in the study was such that it effectively arrested what may be regarded as the final stage of the socialization process, namely the preparation of the sons for an adult role, independent of the family.

(d) *The provision of emotional security and stability*

It is considered that a social function of the family is to provide support to individual family members undergoing stress; this would imply the need for secure and tolerant relationships within the family, but in our sample, with two exceptions, the families were so torn by internal conflict that they could not provide any degree of security beyond the material sphere. How far the illness of the patient contributed to this, and how far the lack of such security was a precipitating factor in the illness it is not possible to say, but it is, on the other hand, quite clear that the additional strains imposed by the illness limit still further the possibility of individuals being able to find within the family the comfort and support which they need in order to face the difficulties of the world outside.

In the two exceptions mentioned, both patients were unmarried sons whose illness was severe, bordering on the psychotic. In these cases the bond of love and affection within the family was sufficiently strong to withstand the enormous load placed upon it, but it is significant that the illness in both cases was so severe as to put the patient into a position of virtual helplessness. Neither had been able to adopt the role of an adult wage-earner since leaving school, and their complete economic and emotional dependence on the family had been continuous since childhood. In situations such as these, the protection afforded to the sick member may well be provided at the expense of other equally important functions of the family. It may also, in some instances serve some positive function for other family members, particularly those who need a 'scapegoat'.

Both these families gave a superficial appearance of stability, but this had largely been achieved by a splitting of the family into two halves; father and other siblings on one hand, and mother with patient on the other; both sides were prepared to make this adjustment in order to accommodate the sick member.

One of the most obvious concomitants of illness on the families in the study was the degree of social isolation suffered by patients and their families, though it is difficult to differentiate here, or elsewhere, between cause and effect. It must be questionable to what extent such families are efficient in providing support and protection for its members, or of integrating them into society. Of the total of fourteen families, twelve could be described as socially isolated; the remaining two families (both parents of dependent sons) are not strictly isolated in terms of external contacts because both parents work very hard outside the home and have good work relationships, though in one case these appear to lack any emotional warmth, and in the second case appear to be largely formal, with social agencies. In both instances it is likely that the parents' inability to make warm social contacts has been a contributing factor in the patient's illness rather than an effect of it, as is the case in the remaining twelve families. There appear to be a number of ways in which the total family has dealt with this problem, and a few categories seem to suggest themselves.

Dependent sons

(i) Mother stays at home and remains virtually isolated, but father and siblings tend to spend an increasing amount of time outside the home. This is true in four cases out of the seven treated in the Unit.

(ii) Two cases have already been mentioned above and cannot strictly be said to be socially isolated.

(iii) In one case the family as a unit is so disorganized that it is completely socially isolated, though it is possible that some of its individual members may have social relationships out-

side. This is one of the families referred to earlier in this paper, where the parents actively blame their son's illness for the family disorganization, but whilst they give this as a reason for social isolation ('the neighbours won't have anything to do with us because of him') it is apparent on interviewing that marital discord and the parent's personality structures are more likely to be causal factors in producing social isolation than is the son's illness, though this may have exacerbated it.

Married men

(i) In four cases both husbands and wives lead extremely socially restricted lives; when it is realized that this is combined with complete work failure on the part of the husbands, the degree of isolation in which these families live will be seen to be very great. In two instances the wives go out to work regularly and appear to derive some satisfaction through social contacts made in this way. In a third instance, the wife's attitudes towards work are inconsistent; at times she works, at others she remains at home, often with her family of orientation. It is felt that where the wife works this is regarded as a major compensation and a socially acceptable substitute for unsatisfactory marital relations.

(ii) In three cases the husband's social contacts are closely related to his problem, and his social life is led quite separately from that of his wife's who may, or may not, have a social life of her own, depending to some extent on whether she has to remain at home with the children. In each of these three cases the problem underlying the symptom has been marital discord, but whilst in two of them the husband has chosen to make his social life separate from that of his wife in the third case it is rather because his wife rejects him and refuses to participate in his social activities or let him enter hers. In these situations it is difficult to distinguish between behaviour causing poor marital relationships, and that which results from such relationships. Insufficient is known about the families of patients who did not come for treatment.

(e) *Operation as an informal agency of social control*

In six of the fourteen families treated (three married, three unmarried patients), it could be said that the family has ceased to function in this respect. Such failure is one of the most obvious symptoms of family disorganization and occurred in those families in which interpersonal relationships had so deteriorated that respect for the rights of others or the authority of the family vested in one or both parents had completely disappeared. It is questionable in several instances whether such respect had ever existed and the evidence suggests that the illness of the patient was little more than one contribution among many to the decay of the authority structure. Although in only one case were the children of a patient legally delinquent, anti-social behaviour was widespread, especially amongst unmarried sons, both within the home and outside it.

In the remaining eight cases, however, the family could be said to function with a reasonable degree of efficiency in this respect. It is suggested that this was to a great extent made possible due to the fact that these families were much less disorganized in relational terms than were the other families in the sample, and furthermore, they were of relatively higher social status. They tended, therefore, to place a somewhat higher premium on conformist behaviour, especially in situations where it might be observed by outsiders. So far as inter-familial relationships are concerned, these families were those where considerable clarity existed in terms of role expectation and function, and if behaviour were deviant, it was recognized as such and quickly covered up and rationalized. As may be expected, there was a close relationship between these higher status families, and degree of skill: where the patient was a married man he was a skilled or semi-skilled worker (even though he might have had a work problem), and in the case of dependent sons, though all were work failures, their fathers were in each case skilled or semi-skilled operatives.

It is important to note when considering the effects of the illness on family members, that the objective effects depend to a very considerable extent on each family member's perception of the nature of the problem. This becomes particularly noticeable in the cases where treatment in the Unit resulted in changes in perception. The marital discord of parents of dependent sons has been discussed above: often this centred round mother's and father's different perception of their son, the father generally considering him to be lazy, shiftless, etc., whilst the mother, even if unable to see him as 'ill', generally recognized that this was a problem with which he needed help. In one instance where the son was borderline schizophrenic, the parents denied that any problem existed until he came into the Unit; father saw him as lazy and inadequate, mother made excuses; when both came to recognize something of the nature of their son's illness, their attitudes changed both towards the patient and towards each other. They grew closer together and provided a much more understanding and sheltered environment for their son, and their guilt could, if necessary, be helped in a treatment situation.

Amongst the married patients perhaps the most clear example of changes in perception affecting the family was in the case of a patient whose illness was seen by both husband and wife in terms of the symptom: drink. Although the husband's alcoholism was seen to result in their drifting apart emotionally, the wife put all the blame on to her husband, and the children were brought up in a home where father's drinking was considered the sole cause of disturbance. Later, when alcoholism was perceived by the wife to be secondary to that of personality and relationship problems, both came to see the bad marital relationship as a causal factor contributing to the presenting symptom. Blame was no longer exclusively attached to the husband, and the problem ceased to be his alone—both could now work towards solving it. A final example is to be found in one of the six cases that did not enter the Unit for treatment. A son of rural middle-class parents behaved 'promiscuously' with

young village girls, and much of his other behaviour bordered on the antisocial—stealing from his younger sisters and running up debts. His psychopathy was seen by the family in terms of failure to live up to class norms, and similar behaviour in the case of the seven sons who entered the Unit would not have been a problem in the same way.

IMPLICATIONS FOR TREATMENT

The main importance of this question of perception lies, it would seem, in the implications for treatment. There appears to be a great need to develop awareness of the facts of the illness among family members and to modify perceptions which are making the situation worse. It is clear from our study of these families, however, that it is not possible, nor always advisable, to involve all families in the treatment process, and it must be recognized that even where families are involved, different techniques and degrees of involvement will be necessary.

In the case of *dependent sons*, it is suggested that there are at least three major ways that the family may react to his illness.

(i) *Parental discord and guilt, resulting in rejection of the son*

In such cases this pattern has usually been continuous over many years and it may be that in some tortuous way the son's illness is a means of allowing the parents to function, albeit inefficiently, as is seen in cases where the continued presence of the son in the home allows the parents to avoid facing up to their own marital problems, and the son becomes the 'scapegoat'. In six out of the seven cases admitted to the Unit, the mother worked outside the home and it may be that this is the most socially acceptable way of dealing with their marital problem. In one case this was made explicit; mother said she went out to work to avoid being depressed by the home situation. To involve such parents actively in treatment is not possible either from their own or the hospital's point of view, and it is suggested, therefore,

that the treatment aim for the patient be directed towards his rehabilitation *apart* from his family. This does not preclude, however, the advisability of helping the family (where they are able to accept it) either to understand the nature of the illness, or to understand their son's leaving them.

(ii) *Overprotection of the son—generally by mother—to the exclusion of father*

It may be that in families of this type, a considerable advantage can be achieved by bringing the parents into treatment, but again much will depend on the individual personalities concerned as to how it should be done.* There appear to be some cases where to remove the defences of the mother is to make the situation worse, and from observation of the cases in the study, it seems that this is particularly true where the mother's feelings for the father are so negative that to loosen her bonds with her son is to deprive her of everything positive that she has. To achieve such an end (even if advisable) the mother would have to be taken into treatment as a patient in her own right. Possibly such parents can be helped in a less traumatic way than by active involvement in the treatment situation, for example by individual interviews, or by slowly accepting the changes which take place after the successful treatment of their son, a process which would involve considerable aftercare work. This also implies that in such cases it may be advisable to prolong the treatment of the patient, in order to give the mother a longer period to adapt to new situations and attitudes, so that she will be able to allow her son to continue the process of maturation after his discharge. In the only case where there was no known parental friction, this appears to have been achieved by the father 'accepting' mother's preference for her son and compensating outside the family.

* Family member may attend a weekly group specifically for this, with Doctors and other patients, or in some instances may attend the regular treatment groups. They frequently see the group reaction to them as hostile and there is often little provision for dealing with anxieties thus aroused.

(iii) *Parents deny son's illness or withdraw from it*

An example cited above indicates how much families can be helped to provide a more helpful environment for their sons, once they have come to recognize the degree and nature of the illness. In one particular family, where the son was so seriously disturbed that he could not be kept in the Unit, one interview with the psychiatrist on discharge was sufficient to alter the attitudes of these parents towards their son (though the visits of the research worker had undoubtedly paved the way). It is difficult to understand the precise nature of the change that took place. The psychiatrist's opinions regarding their son's illness were seen by the parents as the 'truth', and the fact that he had been in treatment for a considerable period had afforded them considerable relief from the responsibility of looking after him. They were, therefore, in a more relaxed state of mind, and could better face up to the facts about the patient's illness. Furthermore, despite the severity of his illness, the son had apparently been helped to some extent by his Unit experience, and as a result was able to react favourably to the more helpful attitudes of his parents. The patient is by no means 'well', but clarification of the situation for the parents, as opposed to treating them, has resulted in both parents and son responding to each other and to situations outside the home in more positive ways.

In the case of *married men*, it is suggested that there are at least two major ways that the family may react to his illness:

- (i) *Wife withdraws from husband, allies herself with the children and there is marital discord.*
- (ii) *In childless marriages external contacts of husband and wife become even further restricted. They are then thrown back on each other almost exclusively, and all tensions must be accommodated within the home.*

There were only two childless couples in the sample, and in one of these cases the wife had two married daughters by a previous marriage. This same wife also went out to work, but

although to some extent such work compensated for the bad marital relationship by affording some degree of external contact, the job required considerable control of feeling, and any tensions or frustrations which arose in the work situation were brought home and added fuel to the already explosive marital relationship. In the second case the husband had a severe work problem and the couple were completely tied to the home owing to the presence of their elderly parents. The anger and frustration were not expressed verbally in this family, but were denied and repressed, the husband suffering, however, from crippling psychosomatic symptoms. Both partners were treated in the Unit and the wife subsequently took part-time work, which she found helped considerably to release her tension.

As has been shown earlier in this paper, marital discord and sexual maladjustment was present in all the married patients in our sample, whether as a presenting symptom or as an effect of illness. In such cases it is possible that the successful treatment of the husband could be facilitated by the wife being actively involved in the treatment process. The question then arises as to the ability and willingness of all wives to respond to treatment of the kind offered in the Unit. In one case in our sample, the wife did in fact attend groups regularly for a short while, but she was quite unable to participate actively; her own needs were being met by the quarrelling and aggressive relationship she had within marriage, and any change would involve a willingness on her part to accept joint responsibility for the present situation, and a genuine desire to change. Thus the wife's attitudes and needs may impose an important limiting condition on the therapeutic goals. By contrast, the case referred to above where the husband was an alcoholic, appears to have achieved a considerable degree of success by bringing the wife into groups. By changing her perception of the problem (and his) they were subsequently able to change their attitudes to each other, and following treatment were able, at home, to work out many of their problems in their own way.

This then raises a further point in relation to treatment—the importance of keeping careful observation on the effects of the treatment process itself on the family, both during the patient's stay in the Unit and afterwards. A particularly clear example of this can be seen in the case of the alcoholic patient just mentioned: towards the end of treatment he adopted many of the norms and behaviour patterns which he had come to associate with Unit ideology (Jones, 1956). Husband and wife came closer together in terms of Unit standards of behaviour, but these were precarious norms, and soon broke down when they left the supportive atmosphere of the Unit. For some while the family saw themselves as worse than they had been before treatment, in so far as in those days the wife could blame her husband's drinking habits for everything, but their new perception of the problem precluded this. Then the situation changed once more; they appear to have recognized that the Unit standards of behaviour were not appropriate to their own life situation, but they were able to adapt these to their own particular needs. It is significant that in addition to the visits of the research worker (for whom they had come to have very warm feelings), this family was also visited weekly throughout this period by the F.S.U. worker whom they had known over a long time, and it would seem that these two factors contributed to the ability of this couple to make full use of the Unit experience and to assimilate that part of it which was applicable to themselves. F.S.U. and research became substitutes for therapeutic after-care, and without them the family might well have relapsed or become worse.

An example which possibly highlights the absence of such following up of treatment, concerns a husband whose physical symptoms entirely disappeared and who, at a behavioural level, appears to function in a much more culturally acceptable way. His wife made only one visit to the Unit and attended the family group which proved to be most traumatic, and she has found it virtually impossible to accept or understand the changes which have taken

place in her husband. He, on the other hand, feels that he has come so far on his own, and it is now up to her to help him; he is aggrieved at her lack of pleasure in his 'new' self which in fact is more responsible and independent. The result of this appears to be a slow deterioration of the relationship at an emotional level, and both partners actively demand help from the research worker when she visits. It may well be that better use could have been made of the wife's potentiality for treatment whilst her husband was still a patient, but failing this it seems most essential that support and after-care be available to the couple during the patient's period of rehabilitation in the community.

Study of these families indicates that the psychiatric illness of one member inevitably presents a distinct social problem in that the balance of inter-personal relationships is disturbed. Although many families will deny that this is so, the fact may be observed in a variety of ways, ranging from their changing perceptions and tolerance of the patient's behaviour to subtle changes in the performance of different members in their social roles. The importance of the family in the process of the patient's rehabilitation has been demonstrated by another study being carried out on ex-Unit patients which indicates that where there is a family to which the patient can return, his chances of successful adjustment are relatively greater. The *possibility* of return is the operative factor, for in certain instances it will be a sign of the patient's improvement if, for good therapeutic reasons, he decides not to return.

Whilst understanding and treating the patient's illness, it is vital to consider his behaviour in terms of his relationships within the family; to attempt to treat the family itself in every case would, as suggested earlier, be unrealistic. With the limitation of resources within the hospital, it is not possible to undertake much responsibility for work in the home or after discharge, and it would seem that a strong case can be made out for fuller use to be made of the resources available within the com-

munity outside. This implies the need for much closer co-operation between the hospital and G.P.'s and social workers who may be already in contact with the patient's family. The possibilities open to the family doctor in this respect are considered in a recent article (Model, 1957). The main difficulty would appear to be lack of effective communication between those involved in helping the family, rather than a complete absence of resources beyond the hospital itself. To be effective, however, the attitudes of

the hospital staff will also have to undergo some change, because it is they who will have both the psychiatric skills and the basic information regarding the patient and his family. It is they who must, therefore, accept responsibility for advising and helping those who will be working subsequently with the families, and this may require considerably more active participation and willingness to impart their knowledge to others than is at present recognized.

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THE COLLUSIVE FUNCTION OF PAIRING IN ANALYTIC GROUPS*

BY R. D. LAING AND A. ESTERSON

Bion (1949) has described a most significant phenomenon which occurs in analytic groups. He writes: '...two members of the group would become involved in a discussion; sometimes the exchange between the two could hardly be described but it would be evident that they were involved with each other and that the group as a whole thought so too'. He goes on to say that it is a 'basic assumption' of the pair and of the others present that the involvement of the pair with each other is of a sexual nature.

In the first part of this paper we wish to consider two problems that arise out of our clinical observations† on pairing: the one is what one might call the relation of social to sexual intercourse in pairing: the other is the nature of *collusion*. In the second part, we shall offer an example of pairing from one group.

One perhaps never sees a group where each member addresses himself equally to everyone else. There always emerge, quite quickly, differentiations within the group, of some kind or other. What may be extremely significant is the occurrence of apparently completely open exchanges between all those present, until one notices that two of the group members are addressing themselves to everyone *except* to each other. These two people may activate the rest of the group, and it may become clear that they are doing this in order to avoid an explicit relationship developing with each other. They constitute what may be called a *concealed pair*, the significance of the one person to the other being apparent in their careful avoidance of open and direct involvement with each other in front of the therapist. The fact that open pair-

ing is sometimes avoided in this way alerts one to the latent anxieties that are involved in the formation in this group pattern. It is not surprising also that it has been the case that such avoidance of relationship in the group has gone along with an actual 'affair' or friendship which has been conducted outside the group, and kept secret as far as possible from the therapist and other group members.

When pairing is manifest it may take practically any form. For instance, the exchanges between the two people may be reciprocal, each participant treating the other as an equal: or, one may be dominant, the other submissive, one active, the other passive, and so on. The exchanges of the pair may consist in continual sharing of similar views, or be predominantly argumentative and mutually aggressive. The exchanges may be animated or desultory; in manifest content banal, or extremely sophisticated and esoteric.

We cannot speak of the pair without considering the significance of the onlookers to them, and of them to the onlookers. For the pair it is an exhibitionistic activity. The others are voyeurs whether they choose or not.

The occurrence of a concealed pair (when two people rigorously avoid openly pairing with each other) alerts one, we suggest, to the anxiety that may be aroused in the pair and the others in the group when pairing is going on, despite the fact that pairing is also a formation which acts as a defence against anxiety, and as an attempt to 'dilute' the transference. Two people are engrossed in each other. They exclude the others, yet tangentially they are acutely aware of the fact that they are under careful scrutiny. They are enacting something in front of other people. The other group members cannot fail to reveal their ways of dealing with their phantasies about this through their behaviour in this situation. One may

* Revised manuscript received 14 February 1958.

† These observations were made on groups run on the principles described by Sutherland (1952) and Ezriel (1950, 1951, 1952).

affect boredom. Another will become irritated; another will try to break up the pair, either by trying to induce one of the partners to form a pair with him or by intruding between them; or he may appeal to the therapist to 'break it up'. The pair themselves may become anxious. They may express the feeling that they are being 'unfair' on the others, or try to deny the threat they feel from the therapist by persisting defiantly, or they may break off themselves ('...but we are monopolizing the conversation').

Now, if such is the manifest content of pairing, any psychoanalytically orientated reader will be prepared to suppose that here we have a situation which must be activating primal scene phantasies, both in the participants and in the onlookers. It is not possible to prove this; all that one can say is that the material lends itself very strongly to placing this construction upon it. However, we would like to assume that the reader will be prepared to regard this as reasonable. We wish to make a further point on the basis of this supposition.

We have repeatedly noticed how readily the participators and the onlookers do seem to assume that pairing is basically a sexual activity. So plausible indeed is the complete assumption made by everyone that this is so, that it is with considerable difficulty that one has to remind oneself that there is very little of the genuinely sexual about it in *fact*. If it is sexual it is so only symbolically. This is surely very important. It is the group which may act as though pairing was sexual. The group in fact often puts this construction upon it quite openly. Thus an interpretation which falls in with everyone's assumption that pairing is 'sexual' is generally accepted. What is not accepted is that one should question the assumption that it is sexual. Here the group can all suddenly become rabid 'Freudians'.

Bion states: 'Whenever two people begin to have this kind of relationship in the group—whether these two are man and woman, man and man, or woman and woman—it seems to be a basic assumption, held both by the group and the pair concerned, that the relationship is

a sexual one. It is as if there could be no possible reason for two people's coming together except sex.' Now the reality of the situation is, of course, that there are many other reasons why people should come together except sex, as Bion himself mentions. If this is so, and yet everyone behaves as though sex were the only reason, this suggests that sex is being called in as a defence against what is not specifically sexual at all.

It is not as fine a distinction as it may at first appear to be to say that everyone pretends, rather than assumes that the pairing is basically sexual. We have had occasion to observe real sexual feelings and real conflict and guilt arising from these feelings between two people in a group—both between two men, and between a man and a woman. The whole effect, the whole tone and atmosphere of the group, is quite different in these circumstances. In the usual pairing the sexual is implicit; it is a resonance of what is going on. The man may be penetrating the woman with sharp, brutal, forceful questions. But he is not actually raping her and has no intention of doing so. One can become so much at home with the symbolical, that one forgets that it is not the reality. Pairing is, or can be, a semblance, a token, of a sexual relationship. It is not an aim-inhibited sexual relationship, in the sense that it is not real five-eighths or nine-tenths of the way and then stops. It is sexual in an unreal way. Indeed the actual chastity of the two members of the pair to each other seems to be a condition for maintaining the pretence. Flirtatious, risqué remarks and sexual innuendo occur, but this must not become too serious or else it will dispel, by injecting actual sexual colouring into the manifest content, the aura of 'as if' which can be one of the most elusive elements in the experience and yet one of the most significant.

When the pairing is 'as if' sexual, the group is achieving at least two things. By pretending that it is sexual, its members are seeking to deny that it is sexual 'really'. Yet they are also denying that it is non-sexual. If it is sexual there is a sense in which it is implied that no further

questions need be asked. Sex is regarded as its own *raison d'être*. Nothing more need be said. We are so accustomed perhaps to think that if there is any free-floating guilt, it can be found its true home once we see that an ambiguous sexual practice is going on. It is clear that the pretence of the sexual to deny its seriousness, or the exhibitionistic nature of the pairing (Ezriel, 1952) can be a means of side-stepping guilt. The central point of some pairing is certainly the fact that the sexual aspects of it are rendered explicit rather than 'kept in the dark' as an expression of the anxiety of the pair lest they be accused of having been engaged in a secret sexual relationship.

Now, the ambiguous equivocal nature of pairing will become clearer at this point if we can understand the nature of the collusion that is going on. But first we require to make a few remarks on the nature of collusion. This can be done for the present purpose by considering a problem which, by its very nature, cannot arise out of direct observations within any form of two-person psychotherapy, namely the spontaneous response of one person to being seen by another, not as he is, but as a figure in that other person's phantasy system. This requires a slight change of focus from the foregoing discussion, but it will become apparent that this shift of focus belongs in fact to the problem before us.

If we think of it for a moment, it becomes apparent how direct observations as to the way a person acts in this position can never be made in a two-person situation and hence can never be made in individual (two person) psychotherapy. In two-person psychotherapy the therapist is aware that the patient is continually seeing him in the light of one or other of his phantasy objects. He regards it as part of his job to point this out to the patient. The therapist is enabled to make such an observation to the patient by contrasting the identity he knows himself to have as a real person, with the person the patient is taking him to be. If the person, he, the therapist, is convinced he is, does not coincide with how he is being regarded by the patient, he points out that the patient is not

reacting to the real person he is, but to a phantasy figure of what he is. The therapist in a group interprets on the same principle, but in a group he is able to observe a process which he can never observe in the two-person situation. He observes the various responses of the members of the group to being treated as objects of other people's phantasy systems and not as they are in reality.

This is the one situation the 'perfect' therapist can never observe in individual psychotherapy, because he (ideally) is always seeing the patient as he is, as a real person, and not as a figure in his own phantasy system. If and when he lapses from this ideal position, it is in just such lapses that he is unlikely to be able to evaluate the patient's response.

The value therefore of analytic groups in giving information about the feelings and behaviour of a person being treated not as he is, but as a phantasy object of another, can hardly be overestimated, since it is one of the very few situations in which observations of an objective scientific nature on this process can be made among adults, for the nature of the problem precludes the possibility of any observations being made on it unless there are at least three persons (including the observer) in the situation.

The matter would be tolerably simple if it could be adequately stated in the above terms as a preliminary formulation. But we must add a further complication. The patient may be seen by the other person in more real terms than he can see himself. In other words, he may be living out a phantasy of himself, and may not have achieved any adequate realization of who he is as a real person.

When two people are 'in relation' to each other they may, ideally, each see the other as he really is. However, as soon as one person is in relationship with another, both are seen to some extent in the light of the other's phantasy system. There are many reactions to being seen, not as one takes oneself to be, but as another person's phantasy object: for example, anger, anxiety, guilt, indifference, despair. All depend in large measure on how firm a sense of

one's own identity one has. The tendency may be, for instance, either to quarrel in anger or to break off the relationship under pressure of anxiety. A third possibility is more complicated but provides one motive for pairing. The individual feels guilty because he is not what the other person takes him to be. Instead of insisting on his own identity and autonomy, he allows himself to become the *embodiment* of the others' phantasy object. A common form of pairing occurs when each member of the pair is being the embodiment of a phantasy object of the other.

The neurotic collusion is cemented when the other person's phantasy of what one is, coincides with one's own phantasy of what one is. In this case, there is, instead of a mutual testing of reality, a neurotic version of a *folie à deux*. Each has found another to endorse his false notion of himself and to lend this fiction a certain semblance of life. It is some such form of collusion, for instance, that we suggest may lie behind the sexual fiction of certain pairings.

A great deal of the neurotic's behaviour consists either in taking fright at being misidentified by others, in getting angry at this: or, in feeling that he has no right *not* to be what the other person takes him to be. Under pressure from this false guilt, or when the other is projecting some acceptable fiction on to him, he may be prepared to be the embodiment of the other's phantasy: especially if the other is prepared to be the embodiment of his phantasy in turn.

It takes two to make a pair, however, and this formation is always liable to be unstable, partly because it is not always, and not for long, that the others will connive at this. Anxiety or jealousy will sharpen their eyes, and usually one other member will see through what is going on. Another factor in breaking up the pair is the hatred that each has of the other.

Sartre has based a play, *Huis Clos*, on the agony of the failure to maintain such collusion in the gratification of each other's requirements. Three dead people, a man and two women, find themselves in a room together.

The man needs another man or, as second best, a woman who will be able to see him with a man's eyes, and in doing so, accord him the recognition he feels he needs from such a person that he is not a coward. Neither of the two women can or will accord him such recognition; the one cannot do so because she is interested in him only as a possible object for her sexual gratification, while the other, a lesbian, could give him what he wants but will not, since she hates men too much. The two women are equally unable to find anyone with whom they can enter into collusion; the lesbian, since she is with an essentially homosexual man and a heterosexual woman, and the heterosexual woman, since she cannot be heterosexual with a homosexual man and a lesbian. This is a situation wherein the individual is unable to form that type of interpersonal relationship which he feels is necessary in order to perpetuate his particular set of defences against the anxiety and despair which haunt his life. Thus: 'L'enfer, c'est les autres.'

The following clinical data* are not of course offered as a proof of our point of view, but as a sample which may fairly illustrate the sort of material out of which our views have developed.

The group consisted of seven men, aged from 25 to 35. With one exception, they were quite successful middle-class people. Jackson owned a garage; Berkeley worked in his father's grocery business. The exception was Richards, who had failed innumerable examinations and was now living at home with his mother, trying to recoup his energies for a further effort to become a chartered accountant.

The basic assumption operative through the early sessions of this group was that it had come together to be dependent on the therapist. He really should have been telling them what to do, asking them questions, giving advice. When he was simply silent or interpreted, they decided, at the suggestion of

* The data upon which the whole of this paper is based are drawn from complete sound recordings of group meetings.

Jackson, who was apparently the most independent of them, that he must be waiting in order to help them and that their best way to help him to help them was to talk about themselves. He became the substitute leader, asking questions, drawing people out, directing the discussion along the lines of difficulties with women, smoothing over tensions, and speaking a bit about his own feelings, mainly in respect of women. The group as a whole warmed to this, with the exception of Berkeley. He spoke to the others on his own initiative but not very freely, and he never spontaneously approached Jackson: when occasionally asked a question by Jackson he answered laconically. Jackson seemed slightly put out by the fact that Berkeley did not respond to his lead as the others did.

In the fifth session there developed the usual discussion about women, led by Jackson, in which everyone participated except Berkeley. The latter, apparently quite irrelevantly, broke in on this discussion to express with considerable vehemence his dislike of football, and the crowds who went to see football. It was an unintelligent game, and football fans were stupid people with whom he could feel nothing in common. (It had been established that all the other group members went to football matches. Jackson went also, not, however, he had said, for the football, but because he wanted to be 'one of the boys'. Berkeley then went on to say how much he longed to meet someone with common interests, who shared his appreciation of the arts, who was not just the same as all the other dull uninteresting people, beginning with his father, who could not see his true value. Jackson took him up by commenting that artists enjoy discussing art with each other. Berkeley said: 'Yes, I'm a bit of an artist, I like to dabble.' Jackson then remarked that football fans also enjoyed talking football, but Berkeley ignored this, and went on to speak of the appreciation of painting. Jackson said, however, that only very well-educated people could really appreciate art. This was a distinctly unencouraging comment to make to Berkeley, who was very sensitive about his lack

of formal education. However, a precarious *rapprochement* was established when at Jackson's suggestion it was agreed that anyone could appreciate music.

Now Berkeley felt himself to be a superior person with superior tastes, but unconsciously he had phantasies of his inner worthlessness. He felt he had never been able to take in anything good from his parents, since they were 'empty' people as he called them, 'dull and uninteresting'. He had, however, the phantasy that the therapist had everything that he needed. The therapist was strong, educated, understanding and appreciative. He expressed often disappointment that 'in this technique' the therapist did not give him more. That is to say, the nearest he could approximate to a criticism of his idealized therapist was to imply that his 'technique' might be bad ('empty, dull and uninteresting'). By sitting beside the therapist as he did, together with him as it were, silent while the others spoke of women, etc., he attempted to achieve an intimacy with him by means of a silent pairing with him. Moreover, he was quite conscious of passive homosexual longings which he had never actualized, a fact he revealed later in a letter he wrote to the therapist. The others in the group were unaware of their homosexual feelings towards the therapist. The virtually compulsive talking about their relations to women seemed partly a defence against the homosexual intragroup tensions.

Jackson also felt that his parents had given him nothing, or not enough, or the wrong sort of things. He, however, was full of aspirations to be a good husband and parent himself, and a good patient. He wanted to *give* all the time, and had already displayed his need to do this by the role he had assumed in the group. However, he was not aware of this. What he found happened in his life was that to his dismay he found himself hating the person whom he thought he loved (the person he had given things to). What seemed to him to happen (consciously) was that he could not stop hating the person he loved. He was frightened at this point that he would lose this person in some

way. He was accordingly aware of being afraid to love (in his own way) anybody, because of his fear of experiencing the pain of losing this person as a consequence of his hatred.

On the face of it these were two most unlikely people to pair in a group, or anywhere, and yet they continued to occupy much of the time for a number of sessions in complicated exchanges which we shall not attempt to unravel. The pairing we are describing is quite unlike real friendship. Real friendship may indeed arise between two people in a group, but that is another story. Jackson was an independent, hard-headed, matter-of-fact, down to earth business man. He was one of those men who regard themselves as extremely heterosexual but in fact spend most of their time drinking with 'the boys', talking about women. He was in no one's debt and was very generous (emotionally and financially). Berkeley was very different. He had dreams of far-away places where things could be beautiful. People were vulgar and coarse. They knew nothing of the finer things. He hated beer. We said that Jackson had felt that his parents had given him nothing of the right sort of things. What, one might imagine, could Berkeley give him that he wanted, or vice versa?

The reader can see many ways in which each was an excellent foil for the other, by confirming the other in his old system of reference. One thing was clear in listening to them. Jackson was never more Jackson than when he was talking to Berkeley. No one showed Jackson 'in relief' as it were, so much as Berkeley. Conversely, no one revealed Berkeley as the person he was, so much as Jackson. And it was, we think, for this very reason that each contributed his part to the pairing. Each tended to stabilize the other in his specific defences and reaction-formations. Each concealed from the other what could break this up. At least, this was so until Berkeley began to impose a further requirement in beginning to make more explicit the nature of his erotic feelings toward Jackson, which threatened Jackson's unawareness of his own homosexuality.

Till then, the group had behaved 'as if' the

pairing were sexual, thereby denying that it could be *really*, and also denying all the other aspects of the collusion which for the time being they had chosen not to notice. Then Jackson asked Berkeley what he thought about when he masturbated. Berkeley said, after some coaxing, that he sometimes thought about a man. Jackson quickly said that he always thought of women, and immediately asked the others whether they did also. This response was a comprehensive repudiation of Berkeley who, it was clear, wished to take Jackson away both from his female objects and the other men, and have him to himself. This was a clear transgression of the unspoken fact that Jackson would never tolerate any clear expression on Berkeley's part of such intentions.

The other group members each reacted in their own ways to this phase in the relationship. The clearest expression of anxiety was evinced by a patient who had conscious phantasies of his parents having damaged each other, and fears of hurting his wife. He was particularly sensitive to Jackson's aggression to Berkeley. For instance, during one such exchange, in which Jackson was attacking Berkeley for not going to football matches, this patient broke in to say he was feeling quite faint, as he had done the night before when watching a boxing match on television when one boxer was giving the other a terrible beating.

Richards was the only one of the group who seemed to wish that the pairing go on indefinitely. He was an extremely schizoid individual. Once, recently, he had left his books to have a walk in the park. It was a beautiful evening in early autumn. As he sat watching the lovers together, and the sun setting, he began to feel 'at one' with the whole scene, with the whole of nature, with the cosmos. He got up and ran home in a panic. It was a relief when he 'came to himself' again. Identity for Richards could be sustained only in isolation. Any relationship threatened him with loss of identity—being engulfed, fusing, merging, losing his separate distinctiveness. He could only *be*, by himself. The sight of people together fascinated him. It seemed so impos-

sible for him, so remote from what was within his reach that one could hardly say he was jealous in a straightforward sense. His inner self was empty. He longed to be together with someone. But he felt he could not be a separate person if he got attached to someone else. If he was attached to anyone, he would be a clam, or a leech, as he expressed it. He was 'outside' life. He could only be a spectator. When Jackson asked him an objectively harmless question his reply was that he felt his existence threatened by questions, and immediately asked Berkeley what he thought. He could only be a *voyeur* of life. This, we may note, points to the fact that pairing, whether or not it is used predominantly as a joint collusive defence, is nevertheless in a sense a positive achievement. At any rate it implies some measure of freedom from the worst fears of being 'eaten up', or engulfed by another, which can preclude the possibility of attachment to anyone on any terms.

In this group, the existence of the pair was not without value since it was able to bring to light a great deal in the group which could be used effectively. Also, the intrinsic instability of the pair and the fact that the others were relatively active people on the whole who did not use Jackson and Berkeley as a means of looking on and saying nothing themselves indefinitely, enabled the group to remain plastic, never becoming sclerosed into one pattern. In another group, however, a pair can bring a whole group to a standstill for a long period if it happens to offer, for whatever reasons, sufficient gratification or defensive security to everyone without mobilizing guilt or anxiety to any considerable extent.

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GUILT AND THE CONTROL OF PLEASURE IN SCHIZOID PERSONALITIES*

By DON D. JACKSON

It is the thesis of this brief presentation that guilt over masturbation, and *mutatis mutandi* other pleasurable activity, stems in part from the fact that the child dares to determine the nature of his relationships. His providing of pleasure for himself is autonomous, assertive behaviour which particularly in schizoid personalities may be experienced as dangerous activity. Through the mechanism of guilt feelings the child attempts to reinstate the parental control in reality and within himself which in turn may continue the cycle by creating a further urge toward autonomy.

It is a necessary extension of this thesis that the mother of the schizoid fears the loss of control of her child; and for her, masturbatory activity may exemplify such loss of control. The mother's (or parents') reaction to suspecting or witnessing her child masturbating is anachronistic, because she cannot help but translate the activity into her own adult sexual phantasies. As a result, a complex communication is set up in which the child accepts blame for activity that he cannot comprehend because he has no notion yet of the framework for his mother's phantasies. The mother, in turn, may feel very possessive toward the child yet reject it because union connotes sexuality to her. Normal adult orgasmic pleasure consists of an explosive autonomy and, at the same time, a feeling of union with another. It is not satisfactory if it is rape, at one extreme, or a nursing situation at the other. The reconciliation of these two poles are impossible for some mothers, and the conflict is communicated to the child over the issue of masturbation. The

healthy body-pleasure and ego mastery of the activity gets connected with an unclear notion of doing something wrong.

There is more involved, in my opinion, in the masturbation conflict between parent and child than the acknowledged deep-seated cultural prohibition against sexual activity. The additional factor involves control, that is, who determines the nature of the relationship. Whereas parents may want children to grow up and manage themselves, they may have difficulty in letting the child develop mastery or autonomy. In this situation, it is handy if the child develops guilt feelings, since he may then express himself autonomously and reinstate the parents' control of himself by feeling guilty. Guilt, in this sense, is an integrative phenomenon both in phantasy and in the communication that the adult perceives. Ethologists have described 'submissive' behaviour in many animal species that communicates 'You have control of me'. This behaviour in the animal can be life-saving—perhaps the analogy to guilt is not inappropriate. In short, both the parent and the child are reactions to more than the lust component of masturbatory activity.

In order to develop the 'control' framework, I will review some historical and etymological aspects of masturbation and compare this theoretical framework with some of Freud's and Fairbairn's concepts.

Pleasure versus control

It is trite to note that for the infant it is a life and death matter that his parents assume virtually complete charge of his existence. Those activities that the parents do not control, for example crying, defaecation, etc., the parents may define as being 'normal' or usual for an infant so that the infant is permitted to be in

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control of these functions. Obviously, parents vary greatly in the extent to which they have to define who is in control—that is, how much autonomy they can allow the infant just as cultures vary enormously in what kind of activities are considered 'normal' for infants. The anxious young mother who is upset because her child cries 'too much' may be relieved if an expert defines the activity as normal. If an outsider defines the nature of the mother's relationship to the child for her, the conflict over wanting to be in control and fearing seeing herself as controlling may be avoided.

As the infant grows from an 'it' to a 'he' or 'she', the parents increasingly need their definition of the relationship to be observed and acted upon by the child. An interesting phenomena occurs in, for example, those mothers who can permit infants to play with their genitals but who are driven to distraction by the same activity occurring in an 18-month or 2-year-old child. Somewhere along in this period comes the imputation of badness to the child, and the mother's need to control this badness or even to stamp it out. My belief is that the difference in the mother's reaction is caused by her suspicion that said child is phantasizing and not merely producing a pleasurable physical sensation. (The mother who cannot permit even the infant to have pleasurable sensation is probably reacting, via identification, as if it were phantasizing.) Why should fantasy (or the suspicion of it) require the mother to strengthen her control? Perhaps because fantasy involves the independent creation of relationships by the child. *Masturbatory activity may mean to the mother that the child is defining his relationship to her as one in which he can have pleasure separate from his mother.* Even more disturbing, it may mean that he is defining their relationship as one in which he is able to control her as, for example, by obtaining sexual pleasure from her in fantasy. In any case, the mother's suspicion that the child is phantasizing makes his behaviour meaningful, a communication rather than a mere wiggling of some fortunately flexible piece of anatomy.

I suspect that the mother of the schizophrenic, in particular, cannot permit the child to have separate relationships, nor permit him to emit those signals that indicate he is having separate relationships, even in phantasy. On the other hand, she cannot permit him to indicate that he is controlling her in reality or in phantasy (Bateson *et al.* 1956; Jackson, 1957). The mother's dilemma; namely, that she cannot tolerate the thought of being the sexual object of the child's phantasies and cannot tolerate the thought that she is not such an object is avoided as long as she denies those signals from the child which indicate an attempt to define the nature of his relationship to her, and as long as additionally she can deny her wish to control or rationalize it via 'God', 'sin' and 'for your own good'.

Some of the secrecy, on a conscious level, surrounding masturbatory activity even in fairly healthy families may be an avoidance of how to handle such an independent act on the part of the child. As long as both parties pretend ignorance, the authority problem is not encountered. It is not uncommon to see this same issue occurring between husband and wife when they are not clear on who is defining the nature of the marital relationship. For example, a young woman came to her therapist quite upset because she had discovered her husband was homosexual. What she actually had discovered was evidence (presumably a communication from him) that he was masturbating and since it was 'the same thing as being in love with a man', she assumed he was homosexual. This was a very controlling woman who constantly attempted in all areas, including sex, to define her husband's relationship to her. However, although he went along with her on most issues, she could not control an involuntary activity like his having an erection, and more than once he failed her in the clinches. What is especially pertinent is her denial that he might be having heterosexual phantasies. This fact, in addition to her general alarm about his mental state, provides a rather analogous situation to the parent who fears the child will go insane because he masturbates.

Evil, insanity and masturbation

The word 'masturbation' appears to have arisen in the late eighteenth century. Its etymology may be from a combination of the Latin roots 'man', hand; 'sutpra', debauchery; and 'turbo' or 'tupra', motion. Thus we have the connotation of evil doing through manipulation. However, the term 'onanism' preceded 'masturbation' and was in rather good repute until relatively recently. Tuke, an eighteenth-century English psychiatrist, stated: 'Onanism is frequently seen in insanity and often is a cause of it.' The term 'self-pollution' was a popular synonym at the time and appears to have had a similar origin to 'onanism'; namely, the story in Genesis, ch. 38, of Onan, son of Judah and brother of Er. Er married Tamar and was dispatched by the Lord for reasons unknown. The next son, Onan, was ordered by Judah, his father, to 'go into your brother's widow, do your duty to her as a husband's brother and produce a child for your brother'. But as the Bible states further, since Onan knew that the child would not belong to him, he used to spill his seed on the ground whenever he went into his brother's widow to avoid producing a child for his brother. For this disobedience the Lord 'cut him off' and interestingly enough, Judah, the father, was then tricked into impregnating Tamar when she disguised herself as a whore and he came upon her outside the city gates. To me, it appears that Onan practised coitus interruptus, not masturbation, and that the point of the story is not the fact that father cuts off the little boy's penis for playing with it, but that father feels like another son in a matriarchal situation. For example, despite his being head man of the group, Judah is tricked by Tamar into impregnating her and in addition has to pay a fee of some fine young goats for the privilege. Thus the woman has been indirectly responsible for determining the fate of Er, Onan, and the powerful Judah.

Some scholars feel that Onan and Er refer to deceased tribes of the Judahites, and if one dwells for a moment on the facts of that period

the Biblical passage makes good sense. The Judahites were nomadic tribes and by reason of circumstances were matriarchal. The men roamed the country side with their flocks while the women maintained a fireside of sorts, presumably an oasis, and raised the children. Evidently, the men returned to camp in a staggered fashion and would cohabit with any available female. The importance of the father was thus lessened since he was not wise enough to know his own child, and since the life or death of the tribe depended upon the woman's getting impregnated and rearing the children. It is obvious that masturbation, zoobestry, and homosexuality would be threats to such a way of life and it may have been to point up such a lesson that the story of Onan originated. Onan was killed for defiance—he refused to accept a communal woman for purposes of furthering the tribe.

There is another story that is perhaps pertinent in a consideration of masturbation myths, the Greek tale of Narcissus. Certainly the term 'self-love', 'self-abuse' and 'Narcissism' are linked in our psychiatric vocabulary, and Freud's choice of this legend for the coining of his term 'Narcissism' may have been overdetermined. You will recall that this is also essentially a matriarchal story in that Echo, one of the Oreades, the mountain nymphs who were tall women and ardent hunters, falls in love with a youth called Narcissus and wastes away to an echo when he spurns her. Narcissus, in turn, is punished by Aphrodite for refusing the maid and is caused to fall in love with his own reflexion, which he sees as a fountain goddess looking up at him from the water. Some accounts have him hearing Echo's voice as he steadfastly gazes at his own image. The fact that he is described as youthful and pale, that he wastes away, and that he changed into a flower which grows from seeds impregnated into the ground are all perhaps relevant to our topic. To overstate the case, when the man refuses to define his relationship to the woman in terms of her having control via the progeny, she becomes the embodiment of emptiness and nothingness—an echo—and he destroys him-

self in the process. Note that additionally by virtue of the nice touch of having Narcissus hear Echo's voice and seeing his image as a fountain goddess we have analogues to a psychotic state. Narcissus further reveals his schizoid nature by the fact that what really destroys him is his attachment to the woman in himself—the internalized maternal object which he projects as a reflexion from the fountain.

Medicine has done its share to perpetuate myths about masturbation. In 1760 Andre D. Simon Tissot published *A Treatise on the Diseases Produced by Onanism* which many authorities regard as having caused a long-lasting condemnation of masturbation. It was re-published as late as 1832. In 1932 Malamud and Palmer reviewed the literature and a series of their own patients in a very thorough study on the role played by masturbation and the causation of mental disturbances. They concluded that in one group of patients masturbation was responsible for the mental illness since it caused a conflict between the ability to stop the habit and the fear of its consequences. Among other disturbances they felt resulted from such conflict were neurasthenia, depression, hysterical manifestations, and paranoid episodes. They felt psychotherapy was definitely a benefit in such cases.

Freud appears as a somewhat equivocal figure in the masturbation-mental illness question. Although he made lasting original observations regarding masturbation, he does not seem to have taken a clear stand on sanctioning the activity. It is understandable that Freud would not have been free to do so when the culture he lived in is considered. However, Freud's theory itself provides additional reason why the practice might be regarded as harmful. In his paper 'On the Passing of the Oedipus Complex' (1924) Freud states that a conflict is established in the little boy over the desire for the love object (mother) fear of losing the penis, and this conflict is solved by his renouncing the love object in favour of the penis. In the little girl, Freud feels, external intimidation leads to the renouncing of the Oedipal desires

and the cessation of masturbatory activity during latency. Hushka (1938) has confirmed Freud's idea that the main intimidating threats in both sexes come from women. Dr Hushka studied information regarding masturbation that was obtained from the parents of 320 problem children. One hundred and twenty-eight of the children's parents stated that the child masturbated and 85 % of the children were severely threatened in order to stop the activity, 67 % of these threats being physical in character. The majority of threats were made by the mother.

If Freud's theory is altered from regarding the organ (penis) as the crucial element to putting the emphasis on the feeling state (assertiveness, control) there need not be separate theories for male and female. Freud himself (1924) equates masturbation and enuresis. Unfortunately, he did not follow this lead to its natural conclusion, namely, the emphasis on assertion and defiance of the parental prescriptions and the consequent guilt feelings.

Psychoanalytic theory

According to Freud, we have to discriminate between two kinds of feelings of guilt. One makes itself known as social fear, fear of outer authority; the other is a fear of inner authority or pangs of conscience. The feeling of guilt originating from the fear of outer authority coincides with the fear of the loss of love. The fear of the inner authority coincides with the fear of the super ego. Freud's derivation of the feeling of guilt is well known. The incorporation of the aggression, that is, the turning of the destructive instincts against the ego, originates the feeling of guilt and makes its appearance in the ego as an unconscious need for punishment. For this reason he has equated the term 'feeling of guilt' with the concept of 'the need for punishment'.

According to Freud's theory, especially as developed in 'Totem and Taboo' (1912) the Oedipus complex is the source of man's sense of guilt and of his morality. Freud supposes that in the primal horde the father was murdered, dismembered and devoured by the

sons. Having perpetrated this act they were seized for a longing for the father whom they had thus lost. This longing was converted into dread of the community which is another term for sense of guilt. As time went on and the longing for the father recurred, being the expression of unsatisfied libido fixated to the father, it became the principal source of the various religions in which the power of the father was re-established by the mechanism of projection. The father was exalted while the son was abased and the primal transgression was redressed. The father's image was revived and in a sublimated form the passive homosexual libido directed toward him attained its goal again. Freud's ideas on the role of the super ego and castration fear can of course apply to any aggressive attempt to achieve gratification—including masturbation.

It is useful to consider Fairbairn's reformulation of the classical psychoanalytic viewpoint (1949) because, to me, it serves as a bridge between the classical view and the concept of control presented here. Taking as his text Freud's statement: 'The super ego is, however, not merely a deposit left by the earliest object choices of the Id, it also represents an energetic reaction formation against these choices', Fairbairn points out that in describing the super ego as a reaction formation against object choices that Freud is describing it as the instigator of repression, and that if repression involves the reaction against object choices *it must be directed against objects and not against guilty impulses*. Guilt, or the sense of personal moral badness, thus becomes secondary to a sense of badness in the object. Fairbairn feels that guilt is thus a defence against relationships with bad objects. The guilt over masturbation thus might be regarded as arising from the relationship of this activity to forbidding internal objects rather than over the necessity of controlling libidinal impulses. Guilt, in this sense, is conjunctive and adaptational since it keeps the bad object preserved (and hence the 'good' object remains 'good') and the conscience pain is offered up as payment for the attempt to control the internalized objects. The sequence

described above is perhaps most impressively seen in the repetitious sequence of masturbation, guilt, promise to abstain, and subsequent masturbation that can virtually destroy the rest of life on earth for the schizoid adolescent. The compulsive repetitiousness of such cycles is difficult for me to conceive of as sexual tension, discharge, and rebuilding of charge, but it can be understood in terms of the wish to control the object and the fear of abandonment that such control brings with it. Some wit once remarked about women that 'you can't get along with them, and you can't get along without them'. Unfortunately some schizoids are in the same position *vis-a-vis* the maternal object. That the body itself as an object is important in such masturbatory conflict can be noted clinically by the following observations:

(1) Rising sexual tension could be handled via nocturnal emissions and then the hand would not be blamed. Obviously this route is not sufficient for such adolescents despite their intense guilt. Perhaps the desire for conscious control of phantasies is an important factor as well as the pleasurable relationship to the body.

(2) Hypochondriacal fears are common in such adolescents and again can be understood, in part, as a struggle of the individual for uniqueness, a private totality, a complete managing of the internal objects and the expiation of such unwelcome ideas of independence. The inability to define the nature of his relationships with external objects results in a chaotic state *vis-a-vis* internal objects and a consequent confusion about who he is and what is him. In this, I am very much in accord with Szasz's ideas (1957).

(3) There may be in such individuals a need to suffer self-inflicted physical or ideational pain. Tics, blushing, excoriations and physical pain and shame are common and have not only an exhibitionistic and self-punitive aspect but may also be unconscious attempts of the individual to define who he is and where he is. In a certain sense, the ability to inflict pain upon oneself, bear it, and feel it subside is much like drawing a line around one's property to declare what is his. That this kind of definition is un-

acceptable to the mother can be noted clinically in patients who pick their noses, scratch or break out in tics. Such activities may goad the mother to frenzy and she may accuse the child of trying to upset her.

(4) The attempt to use one's own body as a substitute for a lost object is discussed below.

To summarize the application of Freudian, Fairbairnian and control theories to masturbation, we might say that they agree that: (a) It is the phantasies that are important and guilt about the act serves as a displacement and hence a safeguard for repression. (2) Problems regularly connected with over-frequent masturbation are conflicts centring about the lacking satisfaction and there is an expectation of punishment for this aggressiveness.

The differences in the theories are that the non-Freudian theories deal with objects—by Fairbairn as intrapsychic occurrences, by the control theory as intrapsychic occurrences directly related to real objects. The latter is thus but a logical extension of Fairbairn's work. For example, the dilemma posed by the child's attempt to define the nature of his relationship to his mother can be understood when one postulates that she is also an object within him. As one schizophrenic patient stated: 'In the end we hurt only ourselves.' Non-schizophrenics may experience the dilemma as a fear of going crazy because of masturbation. Such fear may develop when the individual is growing closer, socially and physiologically, to making his phantasies become reality of a sort. Going crazy may represent the wild gratification of every whim, and at the same time it forces 'them' to take care of him via the punishment of incarceration in an asylum.

Masturbation, auditory hallucinations and abandonment

There are two clinical observations that have struck me as pertinent to my thesis, but neither has been carefully verified. The first is that although the auditory hallucinations of a schizophrenic may accuse him of all manner of possible and occasionally improbable anatomical combinations with others, they do not

accuse him of being, in effect, 'a dirty masturbator'. If this observation is true, then the following speculation might be pertinent: the voices accuse the schizophrenic of activities in which there is an object other than himself. When his own body is the object, the activity, masturbation, provides a frame which states, 'my phantasies are not real' or 'this is only masturbation'. Though a forbidden activity, masturbation may be a *necessary* activity (apart from the sexual gratification) in order that the transactions that occur as masturbatory phantasies do not become thoughts which frighten one lest they take over and result in action. According to this explanation then, masturbation does not result merely from the sequence: tension—fantasy—sexual excitement—masturbation—relief of tension. But in schizoid people there is also the cycle: fear of abandonment—attempt to define the nature of the control of one's relationships—fantasy—increased fear of abandonment—the necessity to define the message as 'only masturbation' (solitary activity)—fear of the autonomy of self-pleasure—guilt and hence reinstatement of the significant other's control, and so on.

The second observation concerns the correspondence between the individuals' feeling abandoned and masturbatory activity. In my experience, it is common for schizoid people when they find no one at home to experience a sudden masturbatory urge. Such urges are common also in stressful situations like final examinations and figuring out their income tax. In understanding such phenomena it is helpful to postulate internal and external objects and the dilemma that controlling them produces. For example, the schizophrenic individual who finds himself alone may experience the phenomena as if he had been abandoned. He immediately turns for gratification to the internal objects which were once external love objects and forces them to gratify him, via fantasy, as well as the taking over of a pleasure-giving function himself, in reality. Often he will experience a momentary return of euphoria only to run headlong into his past and thus to experience guilt and self-abnegation. Now

you see, he has behaved in a fashion so that he deserves to be left and is again threatened with abandonment which he now attempts to handle by shame and guilt. The healthy adolescent may also react to abandonment with a masturbatory urge but he finds some comfort in his own body much as a child stroking his blanket while he chews on a corner of it. Stressful situations may produce a masturbatory urge for additional reasons than mere release of tension. The threat of failure or of success, the urge to defy the authorities by cheating or refusing to comply may light up the old problem about who determines the nature of the relationship. The conflict is acted out in miniature through the masturbatory act and the individual may be freer to take his examination or pay his full pound of flesh.

The concept presented is built around the theme that it becomes increasingly important to the individual from infancy onward to determine the nature of his relationships. The schizoid individual is handicapped in such efforts by his relationship to his mother who is controlling, but who must not be regarded as controlling. The child must therefore develop covert means of handling pleasurable or assertive activity and masturbation becomes an especially meaningful situation because:

(1) Pleasure is obtained from one's own body but guilt feelings make restitution for the assertiveness or defiance involved.

(2) The secretive, bad behaviour provides a frame for phantasies that labels them as *only phantasies*. Disgust cloaks any connexions with real wishes and guilt washes over any notions of assertiveness or mastery.

(3) Although masturbation can be an attempt at mastery, at ego growth via a differentiation of one's self from them, it leads to a fear of abandonment and a wish for outside control. Whether there is an appeal to God or one's better nature, a new cycle will be set in motion and a new attempt to determine the nature of one's relationships.

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Dr Frieda Fromm-Reichman expressed similar ideas to these at a lecture given shortly before her death. She felt that we had conceived the idea independently and urged me to continue to explore it. I am indebted to her, and I would also like to express my appreciation to those who constructively criticized this paper, especially Dr W. Ronald D. Fairbairn and Dr Judd Marmor.

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A COMMENT ON DR BOWLBY'S 'ETHOLOGICAL APPROACH TO RESEARCH IN CHILD DEVELOPMENT'

By LAJOS SZÉKELY*

Though the extrapolation of concepts, research results and hypotheses from ethology to psychoanalysis has some advantages, this naturally also involves certain risks (Spitz, 1955). In general, three arguments may be advanced in favour of 'intercommunication' between the two sciences.

(1) We may expect that certain psychoanalytic hypotheses relating to the theory of instincts may be either confirmed or modified by experimental research with animals. When both psychoanalytic experience and ethological pre-research confirm 'consonant' inductive positions, confidence in induction will be strengthened. Descriptions of 'appetitive behaviour', which occupy a central position in the development of instinctive action (Lorenz, 1950; Tinbergen, 1950; Thorpe, 1956), may increase our reliance on the doctrine of the libido. The concept of 'imprinting' appears to be a description of the analytical theory of the critical phases of ontogenesis and of determination.

(2) When we, for example—whether this is justified or not—ascribe a 'lower scientific status' to psychoanalysis than to ethology: then the confirmation of a psychoanalytical induction by ethology confers a 'higher scientific dignity' on the analytical hypothesis.

(3) Intercommunication between psychoanalysis and ethology can make analysts aware of much that is concealed, or, to state this more objectively: we indicate hitherto unknown facts and prepare ourselves for their discovery.

In his article on 'An Ethological Approach to Research in Child Development' in this *Journal* (1957), Bowlby adheres to an old and honourable psychoanalytic tradition. Bruno (1920, 1926) and Hermann (1923, 1926) were

the pioneers in this field, and as early as the beginning of the 1920's they started to establish psychoanalytic theory on a broader biological basis, by incorporating the findings of animal psychology and ethology respectively. They were joined by Bally (1933) in the 'thirties. Bowlby, however, expects too much from the ethological approach, when he states that it will raise the scientific status of psychoanalysis.

Bowlby assigns to psychoanalysis a lower scientific status than, for example, to ethology or to learning theory, because 'experimental method is conspicuous by its absence', and because the concept of instinct is inadequately defined. What constitutes a science is a very intricate question, which even those specialists who are concerned with the theory of science cannot answer accurately. In any case, the problem is far too complex for such criteria as the absence of experimental methods or the inadequate definition of certain basic principles to prove decisive. Systematic experimentation is only possible where the space-time order of magnitude of the events which are to be studied is within certain limits. But I should like to remind the reader that psychoanalysis is not the only science whose objectives lie outside of these limits. In astronomy there is also only observation and no experimentation. As far as I am aware, however, no one has yet complained that astronomy is less scientific for this reason. Nor should undue importance be attributed, in my opinion, to the criticisms of the learning theorists, that the definition of instinct is *unsatisfactory*. When the ethologists describe instincts as 'species-specific behaviour patterns', we should bear in mind that 'species' in biology is a concept which is not more clearly defined than instinct or libido in psychoanalysis. A symposium, which was recently held by the American Association for the

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Advancement of Science, was compelled to state that no criteria could be found, which, throughout the entire animal and vegetable kingdoms, could uniformly define what constitutes a species. As is well known, the obscurities in the concept of species inspired Darwin, T. H. Huxley and Wallace to carry out the work which culminated in the writing of the most important book of the nineteenth century, Darwin's *The Origin of Species*. To-day, after the lapse of a hundred years, it is still not possible to give a more precise definition of the concept of species; but in spite of this, what a wealth of research, discovery and theoretical speculation has been stimulated by this 'obscurity'.

It is quite likely that certain imperfections in biological science cannot be avoided. An accurate and extensive *descriptive knowledge* of the objects studied, makes the construction of well-defined basic principles more difficult. Learning theory boasts of its formulation of concepts which do not involve discrepancies. The *descriptive knowledge* of how human beings and animals learn and acquire their experience outside of the laboratory is, however, entirely absent. This condition is admitted by even such an authority in this field as Hilgard in the second edition of his standard work (1956). Finally, what is of decisive importance for an empirical science is not whether its basic principles are quite clearly definable, but what the scientists do with the inadequately defined concepts. For a Linnaeus, a Darwin and a Freud the 'vaguely defined basic concepts' function as reliable guides for research in the unknown, in virgin soil.

(3) Intercommunication between psychoanalysis and ethology would prove most fruitful when this led to the 'indication' of new facts which had not been discovered hitherto. Much time will be required until this can be achieved! In psychoanalytic literature there are sporadic signs of such an approach; but where these will lead to is as yet uncertain. More than 20 years ago (1934) Imre Hermann pointed out, and in a more recent work (1957) he has again drawn attention to the fact, that fear of the eye is an

'archaic perception'. What Hermann means by the term 'archaic perception' is in agreement with the key stimulus of ethology. He investigates the further development of this phenomenon in human ontogenesis, and concludes that this constitutes the origin of a specifically human and specifically social affective process, the *sense of shame*. In one of his works (1954), Székely advanced the hypothesis that the human pre-objects of the earliest oral phase are key stimuli in an ethological sense; and from these, the first partial objects develop through individual contact with the mother, through libido cathexis. The two-eyes-nose schema devised by Spitz, which evokes smiling in the first three months, and the eighth-month anxiety in the third quarter is originally a key stimulus. My conception differs from that of Bowlby (1957) in the following points. According to Bowlby the face (two-eyes-nose schema) is a key stimulus for the release of social behaviour. Whereas I maintain that when the child smiles, the face is no longer a key stimulus, but a partial object derived from the key stimulus. Originally, the two-eyes-nose schema was a key stimulus; and in conformity with the views of Darwin, the ethologists and Hermann, it may be regarded as a fear releaser or an 'enemy schema'. On the basis of this assumption, the dynamic of the anxiety of the eight-month child requires reinterpretation. According to Spitz, the child is afraid of a stranger because he feels himself abandoned by his mother. The other theory is, that the child feels that he is deprived of maternal protection because the stranger represents the key stimulus which releases fear. Spitz has contested the truth of this interpretation (1955), but Meili's experiments (1957) appear to confirm it. As a key stimulus, the two-eyes-nose schema is related to oral phantasies, especially to phantasies of passive bodily destruction and of being devoured (Lewin, 1950).

The first smile is the first mastering of the archaic real fear, through the 'enemy schema' acquiring, in course of contact with the mother, a libido cathexis, and becoming a partial object. The first social reaction arises from the matrix of the animal

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SHORT BOOK NOTICES

The Annual Survey of Psychoanalysis. Vol. IV. Edited by JOHN FROSCHE and NATHANIEL ROSS. (Pp. 770. \$12.00.) New York: International Universities Press. 1958.

This large volume deals with psychoanalytic papers published in 1953. These papers are integrated into detailed reviews of various areas, and the main sections deal with critique and methodology, ego psychology and instinct studies, clinical studies, dreams, psychoanalytic child psychiatry, applied psychoanalysis, therapy, training, psychoanalytic studies in psychiatry and psychoanalytic books. 258 papers and 51 books are dealt with in all.

The Neurotic and Psychiatric Aspects of the Disorders of Aging. Vol. 35 of the Research Publications of the Association for Research in Nervous and Mental Disease. (Pp. 307. 68s.) London: Baillière, Tindall and Cox. 1956.

The proceedings of the Association on this topic in 1955 are reported, and include chapters on the biological ageing of cells, the life history of the neurone, brain metabolism, ageing and intelligence, experimental prolongation of the life span, the psychopathology of ageing, drug effects, genetics, epidemiology of mental disorders and ageing, structural alterations in the nervous system, vascular diseases of the brain, and the rehabilitation of the elderly neurological patient. Each chapter is written by an expert in the field, and the panel discussion is recorded.

A Classified Bibliography of Gerontology and Geriatrics. By NATHAN W. SHOCK. (Pp. 525. \$15.00.) Stanford: University Press. (120s.) London: Oxford University Press. 1958.

Almost as many articles have been published relating to the problems and process of ageing since 1948 as in the previous half century. The present volume lists these later references (the earlier ones are given in Dr Shock's original *Bibliography*), and it fills the urgent need for a convenient guide to the most recent references in the field.

The Analysis of Dreams. By MEDARD BOSS. (Pp. 223. 25s.) London: Rider. 1957.

Dr Boss treats the events, objects and experiences of the dream as existing in their own right, not as mere representatives of things outside the dream itself. Following an account of dreams among the ancients, he criticizes notable recent theories of dream interpretation. He goes on to consider a particular dream in detail, and later to analyse the manifold forms of existence that are possible in dreams. He has been much influenced in his thinking by Heidegger and Binswanger.

Journey Through Adolescence. By DORIS ODLUM. (Pp. 186. 10s. 6d.) London: Delisle. 1957.

This small book is designed to help parents and teachers cope with adolescent problems. The chapters deal with pre-adolescence, health and physical changes, the discovery of the self, and the adolescent at home, school and at work; adolescent friendships, pleasures and love, the adolescent and the community and spiritual values.

The Patient and the Mental Hospital. Edited by MILTON GREENBLATT, DANIEL J. LEVINSON and RICHARD H. WILLIAMS. (Pp. 658. \$6.00.) Glencoe: The Free Press. 1957.

This book deals with the social and sociopsychological factors which are important in the recovery of the hospitalized patient. It presents the results of numerous studies conducted by leading scientists in this newly-developing area. Insights into the special situation of the mental hospital have come from these researches, but these are also applicable to the general hospital and other organizations.

Psychotropic Drugs. Edited by S. GARATTINI and V. GHETTI. (Pp. 606. 95s.) Amsterdam: Elsevier; London: Cleaver-Hume Press. 1958.

Written by specialists from various countries, this work contains an up-to-date report of current biochemical, pharmacological, electrophysiological

and clinical knowledge in the experimental and human field. The five sections refer to the biochemistry of normal and altered cerebral function; behavioural effects of psychotropic drugs on animals; the electrophysiological basis of normal and altered cerebral functions; pharmacological aspects of central acting substances, and clinical experiences.

Enuresis—A Clinical and Genetic Study. By BERTIL HALLGREN. (Pp. 159.) Copenhagen: Ejnar Munksgaard. 1957.

This monograph reports a clinical and genetic study of enuresis made on the basis of 229 cases collected from the psychiatric departments of children's hospitals, 173 secondary cases (siblings and parents) and 530 unaffected siblings and parents.

Conclusions are drawn relating to the correlates of enuresis, and emotional factors are considered.

Erogenicity and Libido. By ROBERT FLIESS. (Pp. 325. \$7.50.) New York: International Universities Press. 1957.

Dr Fliess makes a distinction between libido and erogeneity, and proceeds to trace the phases of psychosexual development from infancy, giving detailed consideration to the persistence of earlier in later phases. The volume concludes with what the author terms a groundwork towards a theory of coition, and includes a chapter on partial erotic language.

Psychology in the Soviet Union. Edited by BRIAN SIMON. (Pp. 305. 32s.) London: Routledge and Kegan Paul. 1957.

This book arose out of a visit made by the editor to the Soviet Union in 1955, and contains thirty carefully selected articles which have appeared in Russian journals. These are felt to be broadly representative of contemporary Soviet psychology, and the list of contributors includes all the leading contemporary Soviet psychologists. One of the appendices, by Prof. A. R. Luria, deals with psychopathological research in the U.S.S.R.

Psychosomatics. Edited by J. BOUW. (Pp. 125. 24s.) Amsterdam: Elsevier. London: Cleaver-Hume Press. 1957.

Seven of the Netherlands' authorities in the field discuss various trends and problems in psycho-

somatic medicine. The volume deals with the possibilities and limitations of psychosomatic medicine, psychosomatic aspects of syndrome shift and syndrome suppression, anthropology and psychosomatics, psychosomatic medicine and organic pathology, and the results of psychotherapy in psychosomatic diseases.

Psychobiology. By ADOLF MEYER. (Pp. 257. 50s.) Springfield: Thomas; Oxford: Blackwell. 1957.

The three Salmon Memorial Lectures given by the late Dr Meyer in 1932 are contained in this volume. They are respectively Psychobiology, Pathology and Therapy. In the first lecture Dr Meyer claims that psychobiology can meet the holistic yearnings of our time and can offer the common ground between the humanities and natural science. In the second he presents pathology primarily as an issue of control, and the concept of reaction sets is described. In the third he bases principles of therapy on the idea that normal activities can be used to digest the less normal tendencies. There is an appendix amplifying some of the fundamental terms and an additional chapter on bibliographical information and quotations.

Hypnography. By AINSLIE MEARES. (Pp. 271. 58s. 6d.) Springfield: Thomas; Oxford: Blackwell. 1957.

This book describes a technique whereby the hypnotized patient projects his conflicts in painting, the material thus obtained being used in the patient's treatment. Dr Meares suggests that the graphic expression of a conflict involves different processes from the verbal expression of the same conflict.

No and Yes. By RENÉ A. SPITZ. (Pp. 170. \$4.00.) New York: International Universities Press. 1957.

Dr Spitz devotes this monograph to the beginnings of communication, using the theoretical framework of psychoanalysis, as well as direct observations of infants, both normal and abnormal; and the newest findings of animal ethology, experimental psychology, embryology and physiology. He integrates the relevant data into a theory of the roots of communication, both verbal and non-verbal.

Sigmund Freud: Life and Work. Vol. III. The Last Phase 1919-1939. By ERNEST JONES. (Pp. 536. 35s.) London: Hogarth Press. 1957.

This is the third volume of the important trilogy completed by the late Ernest Jones on the life and work of his teacher, colleague and friend. It deals with the new ideas produced by Freud in his last twenty years, with the turbulent development of the psychoanalytic movement during these years, and with Freud's own history in Vienna and later in London.

Contemporary Approaches to Cognition. By JEROME S. BRUNER and others. (Pp. 210. 32s.) Harvard: University Press; London: Oxford University Press. 1958.

This volume reports the proceedings of a symposium held at the University of Colorado. The contributors include Egon Brunswik, Leon Festinger, Fritz Heider, Karl F. Muenzinger, Charles E. Osgood and David Rapaport, as well as Dr Bruner. These authorities have built their material round a definite plan—the first paper presents a historical introduction to the topic of cognition. The next four represent particular viewpoints in psychology, summarizing conceptual problems and empirical findings. Finally, a synopsis of the symposium is outlined, as well as future trends in the study of cognition.

Psychotherapy of the Adolescent. By BENJAMIN H. BASLER. (Pp. 270. \$5.00.) New York: International Universities Press.

The neglected area of psychotherapy with adolescents has been dealt with by a number of experts in this comprehensive work. Various chapters deal with work in private practice, in school practice, at the in-patient and clinic levels. There is an extensive discussion, a recording of a psychotherapeutic interview with an adolescent and attendant comments, and a chapter on the schoolmaster's viewpoint. Dr Basler adds a final summary.

Homosexuality: Its Nature, Causation and Treatment. By CLIFFORD ALLEN. (Pp. 143. 15s.) London: Staples. 1958.

This book discusses the problem of homosexuality, its incidence, cause, social significance, prevention

and treatment in an open and frank manner. A chapter on the Wolfenden report is included.

A Study of the Major Psychoses in an English County. By MICHAEL SHEPHERD. (Pp. 159. 25s.) London: Chapman and Hall. 1957.

The third of the Maudsley Monographs, this work deals with the changes in the number and type of patients admitted to a mental hospital between 1931-3 and 1945-7. Conclusions regarding social, medical and administrative factors are drawn.

Light, Colour and Vision. By YVES LE GRAND. (Pp. 512. 63s.) London: Chapman and Hall. 1957.

Translated from the French, this volume deals with the physical and physiological aspects of the subject.

Psychotherapy: A Modern Theory and Practice. By E. LAKIN PHILLIPS. (Pp. 334. 30s.) London: Staples. 1957.

The author presents a new concept of psychopathology and psychotherapy, which is 'in striking contrast to conventional ideas derived from Freud'. He regards behaviour as a function of conscious choice rather than as a function of what lies in the mind's depths, and his theory is primarily one of 'interference'.

Instinct in Man. By RONALD FLETCHER. (Pp. 348. 40s.) London: Allen and Unwin. 1957.

Dr Fletcher examines the whole doctrine of human instincts and argues for the reinstatement of the theory of instincts in psychology. Apart from the work of the older theorists the recent findings of the comparative ethologists and the instinct theories of psychoanalysis are examined. Finally a comprehensive theory of instincts is presented.

Child Psychiatry (3rd edition). By LEO KANNER. (Pp. 777. 63s.) Oxford: Blackwell. 1957.

This standard text-book has been revised in the light of recent advances. While additions have been incorporated into the text, the general plan of the second edition (1948) has been maintained.

Stuttering. By DOMINICK A. BARBARA. (Pp. 255. 25s.) London: Hutchinson. 1958.

Subtitled 'A psychodynamic approach', this book describes many of the phenomena of stuttering and stammering, illustrated with case-histories. The author discusses the various methods of treatment and such facets of the problem as magic and self-effacement in stuttering. Influenced by Karen Horney, he describes a method of 'self-realization and the ultimate conquest of the stutterer's neurosis'.

Katharsis. By C. W. VAN BOEKEL. (Pp. 270.) Utrecht: De Fontain. 1957.

Following the chronology of Jaeger and Nuyens, this philological study (in Dutch) is an attempt to reconstruct Aristotle's doctrine on the psychology of the emotions, in so far as it refers to the notion of catharsis.

Hypnosis in Health and Sickness. By GORDON AMBROSE and GEORGE NEWBOLD. (Pp. 196. 15s.) London: Staples. 1957.

An account of hypnotism is given in this work, together with its application in the treatment of various neurotic conditions. Two chapters on suggestion and hypnosis in midwifery are included.

Hospital Treatment of Alcoholism. By ROBERT S. WALLERSTEIN and Associates. (Pp. 212. 42s.) London: Imago. 1957.

Dr Wallerstein and his colleagues present in this volume the results of a two and a half years' study on the relative efficacy of four recognized methods of treating alcoholism (antabuse therapy, conditioned reflex therapy, group hypnotherapy and closed ward milieu therapy). The results have been evaluated by a team of psychiatrists and clinical psychologists, and are related to the personality structures of individual patients.

The Dynamics of Anxiety and Hysteria. By H. L. EYSENCK. (Pp. 311. 32s.) London: Routledge and Kegan Paul. 1957.

The author has attempted to 'elucidate the dynamics of neurotic behaviour' by using the postulates of modern learning theory. The book is illustrated

with many diagrams and photographs; the latter including the various pieces of psychological apparatus referred to in the text.

Variation and Heredity. By H. KALMUS. (Pp. 227. 28s.) London: Routledge and Kegan Paul. 1957.

Dr Kalmus attempts to strike a fair balance between the environmental causes of human variation and the genetical, and to discuss their complicated interaction. The important human differences are surveyed, relatively non-technically, in relation to practical problems such as miscegenation.

Personality, Appearance and Speech. By T. H. PEAR. (Pp. 167. 15s.) London: Allen and Unwin. 1957.

Prof. Pear indicates the innumerable outward signs by which the man in the street judges personality. He discusses the extent to which a man's real character can be assessed by such signs, and also the possibility of the deliberate use of these signs to create a desired impression.

Sex Perversions and Sex Crimes. By JAMES MELVIN REINHARDT. (Pp. 340. 43s.) Springfield: Thomas; Oxford: Blackwell. 1957.

Written primarily for law enforcement officers, investigators, judges and prosecutors, this work consists for the most part of descriptive accounts. The case material is, however, considered from the point of view that the significance that behaviour has for society reacts upon it so as to alter its appearance or to force a substitute expression.

Family and Social Network. By ELIZABETH BOTT. (Pp. 252. 30s.) London: Tavistock. 1957.

An intensive, long-term field study of family relationships carried out by the author and others in Greater London is described in this volume. The study of the research families demonstrated that the type of relationship existing between spouses is associated with the type of social network in which they live, and the nature of this association is explored and discussed in detail. Stress has been placed on methodology and theoretical formulations.

Glory Reflected. By MARTIN FREUD. (Pp. 218. 21s.) London: Angus and Robertson. 1957.

An informal account of the life of Freud as seen through the eyes of his eldest son is presented in this book. It is written in the form of the author's autobiography, with an introduction by Princess Marie Bonaparte.

The Patient Speaks. By HAROLD A. ABRAMSON. (Pp. 239. \$3.50.) New York: Vantage Press. 1956.

The psychotherapeutic interviews of a 32-year old woman suffering from severe eczema have been recorded by Dr Abramson, and verbatim extracts have been presented to illustrate the process of psychotherapy. The material has been appraised in terms of the acting out of the life goals of the parent, and the role of the Cronus complex (after the god Cronus, who swallowed his own children) in allergy is described.

Methods of Group Psychotherapy. By RAYMOND J. CORSINI. (Pp. 251. 49s.) London: McGraw-Hill. 1957.

The first part of this book deals with historical developments, theory and mechanisms of group psychotherapy, a comparison of individual and group therapy, methods and applications of group treatment, actual procedures and processes, the group therapist and an evaluation of psychotherapy. In the second part some major methods are described, including analytic group therapy, non-directive techniques, family counselling and psychodrama. An extensive bibliography is added, as well as a section on visual aids.

Principles of Perception. By S. HOWARD BARTLEY. (Pp. 482. 52s.) New York: Harper and Brothers. 1958.

Perception is treated in this work from the point of view of scientific psychology. It is meant to be 'a decided step in getting away from the common persistent retention of mentalistic concepts and usages'. Freudianism is mentioned on p. 25 and Piaget not at all.

Introductory Psychology. By D. R. PRICE WILLIAMS. (Pp. 203. 18s.) London: Routledge and Kegan Paul. 1958.

The intention of the author is to provide an introduction to modern psychology for social workers. The contents are fitted into the fivefold framework of Motivation, Learning, Perception, Communication and Personality, and are meant to present a background for further study.

Clinical Applications of Suggestion and Hypnosis (third edition). By WILLIAM T. HERON. (Pp. 165. 27s. 6d.) Springfield: Thomas; Oxford: Blackwell. 1957.

This work provides a technical outline for the application of suggestion and hypnosis, and is up to date in that the use of a tape-recorder is described. The author is Professor of Psychology at the University of Minnesota.

Peter and Caroline. By STEN HEGELER. (Pp. 29. 7s. 6d.) London: Tavistock. 1957.

The subtitle of this slim volume is 'A child asks about childbirth and sex', and the Foreword is by Prof. W. C. W. Nixon of University College Hospital. The pages contain delightfully frank suggestions for parents' answers to the embarrassing questions asked by their children, and is clearly and abundantly illustrated by equally frank line drawings. It is suggested that the book can 'be left to the children themselves to pick up as they feel the desire to return to the subject and become more familiar with it'.

Handbook of Social Psychology (revised edition). By KIMBALL YOUNG. (Pp. 632. 35s.) London: Routledge and Kegan Paul. 1957.

The revised edition of this well-known work is divided into two main sections. Part I deals with social interaction and the fundamental interrelations of the individual, group, and culture. Part II deals with collective behaviour. The book has been almost entirely rewritten for this new edition.

A Short History of Psychotherapy. By NIGEL WALKER. (Pp. 185. 25s.) London: Routledge. 1957.

The author, who was trained in Philosophy, has brought his journalistic skill to bear on the problem of the development of modern psychotherapeutic methods, and has attempted to describe the main divergences in current theory and technique.

Psychiatry and the Criminal. By JOHN M. MACDONALD. (Pp. 227. 42s.) Springfield: Thomas; Oxford: Blackwell. 1958.

This book aims at providing a practical guide to the psychiatric examination of the suspected criminal. In addition to a brief though dynamically-oriented chapter on the origins of criminal behaviour, it includes chapters on tests of criminal responsibility, the psychiatric examination for the criminal court, the Ganser syndrome, simulation of insanity, narco-analysis, amnesia, epilepsy, psychopathy, alcoholism, sexual offences, juvenile delinquency, psychological tests, psychiatric evidence and treatment. The legal aspects are all considered in relation to United States law.

Pain and Pleasure. By THOMAS S. SZASZ. (Pp. 301. 30s.) London: Tavistock. 1957.

Prof. Szasz analyses the basic concepts involved in the sensations of pain and pleasure and in our communication of them. Utilizing the insights of psychoanalysis, sociology and philosophy, Dr Szasz demonstrates the doubtful value of such distinctions as 'real' and 'imagined' pain or 'physical' and 'intellectual' pleasure. He illustrates these points by examples drawn from his own clinical experience with cases of hypochondriasis, false pregnancy, phantom limb pain, sexual deviation, and hysterical anaesthesia.

Personality and Temperament. By SOLOMON DIAMOND. (Pp. 463. 48s.) New York: Harper; London: Hamilton. 1957.

This work takes as its central theme the development of individuality in the normal personality. It is more than an account of the various personality theories usually presented to psychology students in that it includes a discussion of the dynamic aspects of identification and the self-concept, and stresses the importance in the human personality of intellectual motivations and the function of concept formation.

On Not Being Able to Paint (second edition). By MARION MILNER. (Pp. 184. 21s.) London: Heinemann. 1957.

This second edition of a well-known work is much revised and now has added to it a foreword by Anna Freud. The author, who is a practising psychoanalyst attaches her own name to this new edition (she previously used the pseudonym of Joanna Field), and recounts her personal experience of what happened when she turned away from conventional methods of learning to paint to free drawings. The resulting insights are of interest in relation to the creative process generally, and a newly added appendix, intended primarily for psychoanalysts, outlines some theoretical aspects of creativity.

The Hangover. By BENJAMIN KARPMAN. (Pp. 531. 72s.) Springfield: Thomas; Oxford: Blackwell. 1957.

The subtitle of this book is 'a critical study in the psychodynamics of alcoholism'. It is written in the form of fourteen case-histories which are then discussed; following this certain general statements are made. The cases are described in great detail, and each is illustrated by vivid drawings by Wesley R. Wilken. It deals with a topic which is rarely mentioned in psychiatric text-books—the hangover, with its attendant problems. In particular, it studies the ever-recurring problem of the alcoholic's guilt-feelings.

Memory: Facts and Fallacies. By I. M. L. HUNTER. (Pp. 185. 3s. 6d.) London: Pelican. 1957.

This is a popular scientific account of the phenomena of memory, written by a lecturer in Psychology at the University of Edinburgh. It includes chapters on repression, imaging and on improving memory.

Chronic Schizophrenia. By THOMAS FREEMAN, JOHN L. CAMERON and ANDREW MCGHIE. (Pp. 158. 21s.) London: Tavistock. 1958.

This work has a preface by Anna Freud. It is the outcome of a research project carried out by the authors with hospitalized schizophrenic patients who were studied intensively over two years. The authors are led to agreement with Federn's con-

tention that schizophrenia is a disease of the ego, and proceed to develop the view that schizophrenic manifestations are inevitable elaborations of a weakened sense of self-discrimination. Following an account of the disturbances in ego-function, elaborated from a psychoanalytic standpoint, the treatment of chronic schizophrenia is discussed.

The Hidden Persuaders. By VANCE PACKARD. (Pp. 275. 18s.) London: Longmans Green and Co. 1957.

Written by an experienced journalist, this work draws attention to the many techniques used in the

United States to promote higher sales. These techniques aim at making use of a knowledge of unconscious desires in the prospective purchaser, and are described in such chapters as 'The built-in sexual overtone', 'Self-images for everybody' and 'The psycho-seduction of children'.

High Arterial Pressure. By F. H. SMIRK. (Pp. 764. 75s.) Oxford: Blackwell. 1957.

This is a major work on all aspects of hypertension and includes comments on the psychogenic aspects of the disease and a complete chapter on 'Psychosomatic aspects of high blood pressure'.

THE USES OF FREUDIAN THEORY IN PSYCHIATRY

BY EDWARD GLOVER*

The fact that many of this audience are in process of launching themselves on a psychiatric career tempts me to look back over thirty-five years of psychoanalytical experience, in order to reformulate the relations between psychoanalysis and psychiatry. I should like in particular to indicate what in my opinion are the services psychoanalysis can render the young psychiatrist, irrespective of whether he should himself ever undergo technical training in psychoanalytical therapy; in other words, what degree of clarity of ideas and therefore professional peace of mind can the psychiatrist attain by familiarizing himself early in his career with the systematic concepts of psychoanalysis.

Before doing so, however, I think it is desirable to recall the ever-widening connotation of the term psychiatry. During these thirty-five years psychiatry, which originally denoted mainly the study, certification and supervision of the manifestly insane, has become synonymous with abnormal or clinical psychology, and hence comprises also the pre-psychoses, and hence comprises also the whole range of neurotic and character disorder, the whole range of inhibition or perversion, the whole range of sexual and social (including working capacity), and the whole range of psycho-somatics, with which are now linked the organic reaction types or pathoneuroses. Here, if he cares to do so, the general psychiatrist can call a halt, and delimit his professional frontiers, insisting, if he likes, that he is no more than an empirical student of classical mental disorders. With three exceptions, however. The child psychiatrist, the forensic or delinquent psychiatrist, and those adventurous spirits who seek to deal with marital difficulties, cannot hedge themselves behind 'symptomatic' limits.

Not only so, the increasing employment of social workers either as ancillaries or allies in the work of diagnosis and treatment represents an extension of activity which goes well beyond the usual clinical range of psychiatry, and to which indeed there is no foreseeable end.

In view of these extensions of the range of clinical psychiatry we are entitled to formulate a preliminary generalization, viz., that whoever studies abnormal psychology is under obligation to study normal psychology also, and that in both instances he must also study social psychology. Expressed in terms of training, this means that at the same time as he becomes a psychiatrist the student must become a psychologist.

Four questions, however, immediately arise: first, what kind of psychology will he study; secondly, will he approach this wider field with or without a theoretical frame of reference? Further, if he does adopt a theoretical frame of reference, what kind of theory should he exploit? Finally, will he proceed from the normal to the abnormal or from the abnormal to the normal? To be sure, it is possible to ignore all theories and approach the subject armed solely with a capacity to correlate descriptive observations. But, as in the case of a recent attempt to study delinquent behaviour *de novo*, ignoring all previously established psychiatric findings, this course usually results in the re-discovery of what has been known for at least a quarter of a century. From my own experience I would have said that it is essential to approach psychiatry armed with a theory of mind that will account for both normal and abnormal manifestations. This is especially true of the study of the psychoses, where to have no theory of mind is to be at sea without a chart or compass. Without theoretical orientation the psychiatrist constantly encounters phenomena he cannot correlate except on a

* A lecture given in the Maudsley Bequest Lecture Course, 14 November 1956.

narrow descriptive basis, which is constantly stultified by exceptions.

I think it will help to clarify these issues if we pause for a moment to consider what distinguishes the psychiatrist from what is now known as the normal or laboratory psychologist, and in particular to distinguish between the aims and motives of the two groups. There may be some here who, like myself, can recall the academic psychologies extant at the beginning of this century, when psychology was still the step-child of metaphysics, and was hampered by the assumptions that mind and consciousness were co-terminous and that the phenomena of abnormal psychology were artefacts, scarcely worthy of attention. What I think struck most of us at that time was the arid nature of their studies, their lack of contact with human nature and their total neglect of the factor of mental suffering. On the other hand, I think you will agree that the legitimate concern of psychiatry is the study of mental suffering or, more accurately, study of types of defence against mental suffering, most of which, as in the case of the psychoses, have broken down and have to be reconstructed.

It is this very fact, I think, which gives us a clue to the aims and motivations of the psychiatrist (assuming of course that he has chosen a profession rather than a career). For there are two main ways in which the mental physician can master or at any rate still the disquiet aroused in him by contact with mental suffering—one is the therapeutic effort to cure the patient and the other is to have a theory of mind by means of which he can understand the nature of the disorder, whether he can cure it or not. And we may say at once that if he elects to carry with him on his professional pilgrimage a theory of mind it must not only account for both normal and abnormal psychology but shed light on *mental conflict*.

Here I should like to dilate for a moment on the *clinical* comforts to be extracted from a skeletal theory of mind (for all mental theories at present are skeletal) apart from that of bridging the gulf between normal and abnormal psychology. First and foremost is its use in the

assortment of data that are often so bewilderingly diversified, the classification, for example, of groups and subgroups of psychotic states. Secondly, comes the correlation of etiological factors in these and other disorders with a view to increasing the accuracy of diagnosis; thirdly, the application of these correlations to the problems of prognosis; fourthly, their use in guiding selection of therapeutic methods, based on understanding of the rationale of the latter. Finally there is the use of a theory of mind in guiding research, as regards both selection of subject and methodology. In short, unless you have a consistent and, however skeletal, all-embracing theory of mind, you are likely to spend a large and harassed part of your professional life looking for one, to the detriment of your pleasure in consultation.

This is the point at which to inquire how far the theories of Freud satisfy these conditions, remembering of course that Freud himself started his study of neuroses and of dreams without any such preconceived theory and only such hints and intuitions as he could derive from his training as biologist, biochemist and neurologist.

Briefly, the development of psycho-analytic theory passed through two main phases. The first of these, which ran from 1893 to between 1916 and 1920, was based on the discovery of the dynamic unconscious, on the study of primitive instincts and on the isolating of unconscious mental mechanisms controlling or inhibiting instinct discharge. All of these were based on the study of the neuroses and of dreams. The first discovery swept away the old assumption that mind and consciousness were co-terminous; the second led to the postulation of unconscious conflict, and the third led to the concept of unconscious ego-defence. At the same time Freud was encouraged through his investigation of dreams to formulate his theory of a *mental apparatus*. The function of this apparatus is to regulate the flow and ebb of instinctual energies (and of the emotions they engender), in order to keep psychic excitation

at an optimum level. The apparatus was conceived of as having certain frontiers or boundaries; a sensory and a motor end corresponding roughly to stimulation and discharge systems; and it was divided into psychic systems defined in accordance with their relation to consciousness.

This basic concept of a mental apparatus was in the main dynamic and economic. It was indeed an excellent description of the infantile psyche during the early months of life, but its structural aspects were not sufficiently developed to be correlated with the total personality of more mature subjects. From the time of his study of melancholia (1916) Freud became increasingly concerned with the structure of the apparatus, and in 1922 formulated his now familiar tri-partite distinction of psychic institutions, comprising the id, the ego and the super-ego. All this is no doubt familiar to you and I do not intend to weary you with time-worn recapitulations. I should like, however, to indicate some of the comforts that can be extracted from this particular theory of the mental apparatus, and would begin with the uses of the old boundary concept of instinct and the later development from it of the id-concept i.e., of an instinctual reservoir, an unorganized and perpetually unconscious part of the total psyche. In brief the id-concept is particularly useful in clarifying some of the confusions that are likely to beset the psychiatrist during and after his qualifying studies. The syllabus of the D.P.M. for example, includes *inter alia* such diverse subjects as genetics, endocrinology, neuro-physiology and neuro-pharmacology, a smattering of normal psychology and of various testing techniques: it also includes clinical study of the psychoses, neuroses, organic reaction types and the psycho-somatoses. After absorbing which the psychiatrist is pitchforked into practice with undigested confusions regarding the relation of body to mind and of the relation of neuro-physiological types of treatment (from drugs to leucotomy) to psychotherapy. Take for example the significance of constitutional or endocrinological factors in dis-

ordered behaviour (whether this is measured by symptomatic or by social standards). Now it is tempting to jump the gap between organic stimuli and behaviouristic discharge, to neglect psychic factors and adopt the ideologies and metaphorical expressions used to describe organic function, or to reach out to the oversimple neurological concept of conditional reflexes, or again to canonize pre-natal factors operating at periods when no psychic organization exists. And the same applies to the approach to organic reaction types. But once you have conceded the existence of a mental apparatus you must always reckon with it excepting possibly in cases of complete amnesia. The concept of the id is, like its forerunner the concept of instinct, a *boundary concept* delimiting one of the frontiers of mind. The constitutional factor (which incidentally the psychoanalyst is the first to recognize, indeed to emphasize) can be thought of as operating *through* the id and therefore subject to and modified by the central mental apparatus which is organized during early development. The id concept is indeed more than a boundary concept; it has also a professional utility: it functions as a professional barrier behind which the psychoanalyst, or psychiatrist or psychologist can tranquilly proceed to mind his own business, which is the study of the various aspects of mental activity.

Regarding the companion concepts—the ego and super-ego, two comments are sufficient for our present purpose: first, the Freudian ego (unconscious and conscious) provides a caption for an orderly developmental series of component structures which are the basis of personality, and permit a structural approach to the problem of reality adaptation. It also provides a series of developmental measures of disordered function in the sense that regression, that primary tendency of the mental apparatus in the face of excessive stress, leads to the executive reanimation of early phases of development to which the ego is fixated. Secondly, the concept of the super-ego places in the forefront of mental development the enormous importance of *guilt*. The isolation of

this psychic system, which reaches from the surface to the deepest unconscious layers of the psychic apparatus, is comparable in importance with the original neurological discovery that the brain consists of two hemispheres. It gives a psychic habitation and a name to the processes of mental conflict. In this sense it is a cope-stone to the theory of mind. It brings mental theory in the closest relation to the phenomena of human life and behaviour, in particular the phenomena of conflict or suffering. In this sense it fulfils that essential condition of a mental theory, namely that it should enable us to trace the influence of conflict operating through the ramifications of both normal and abnormal mental processes.

Naturally the most useful application of these theoretical concepts lies in the clinical field. Here they provide a *sliding-scale of psychiatric values*. The estimation of abnormal or normal mental processes calls in each case for the application of a basic three-point scale. This requirement is met by the now familiar but essentially psychoanalytical distinction of constitutional, developmental and precipitating factors, each of which can be measured in terms of mental stress. In psychoanalytical theory these three terms are paraphrased and referred to respectively as id factors, ego and super-ego factors, and immediate factors of frustration and stress. Diagnosis and prognosis are rendered immeasurably more accurate by the application of this sliding scale, by estimating each disorder in accordance with the weighting of the three basic factors.

Needless to add the psychoanalyst concentrates his attention on the second or developmental group of factors, and justifies himself on the practical ground that only here will the observer find a diversity of etiological factors that will satisfactorily account for the clinical diversity of symptom-formations. To be sure, the psychoanalyst is frequently criticized for the over-elaborate detail with which he describes phases of instinctual development, stages of ego and super-ego development and a hierarchy of psychic defence or discharge mechanisms. But as in the case of organic

medicine, complexity is not a sound basis for criticism, unless it calls for the application of Occam's razor by exceeding the necessities of clinical explanation. There is in any case a practical means by which we can trim any superfluity of etiological explanations. It lies in the assumption that mental breakdown, either total or partial, can be satisfactorily explained only by presuming disorder of *important* instincts, *important* mental structures and *important* regulating mechanisms, operating simultaneously. Even so, the psychoanalytical elaboration of etiological factors is simplicity itself compared with the multiplicity of factors which the descriptive psychologist is compelled to assume, for example, when describing instinctual behaviour or when considering the conditions that precipitate mental illness. Psychoanalysis has indeed rescued the precipitating factor in illness from banality by insisting that in all mental disorders, not excluding the traumatic neuroses, the immediate precipitating agent must be assessed in terms of its capacity (a) to produce regression, and (b) to excite qualitative and quantitative changes in the central mental apparatus, which is *ex hypothesi* unconscious. Only a theory which can indicate stages in the development of instinct and outline the main developmental layers of ego and super-ego structure can provide the means for accurate diagnosis in depth, for prognosis in depth and for depth etiologies.

I have referred earlier to the uses of a systematic theory in estimating prognosis, and it would seem appropriate to refer at this point to its uses in analysing the rationale of *treatment*, in selecting appropriate treatment and in explaining the results of treatment. It is not my intention to discuss here or compare the results obtained by different methods of psychotherapy. We may perhaps agree that both successes or failures can be recorded to the credit or debit of any form of therapy, from magical incantation to 'deep' analytical interpretation (although I am tempted to remark that the technique of magical incantation, although officially frowned on under Western Civiliza-

tion, still persists in many unobtrusive forms in the techniques of hypnotism, suggestion, faith healing and, may I add in the interests of fairness, of inaccurate psychoanalytical interpretation). We may, I think, also agree that in one form or another the analytical theory of transference has come to permeate most individual psychotherapeutic efforts, to say nothing of social therapies. But it is not enough to talk glibly of transference factors as determining the degree of 'accessibility' of any given case, or to be content with the simple classification of positive or negative transferences. We must know what exactly is transferred, and this in turn involves understanding of the various phases of ego and super-ego development, of the serial development of object-relationships and of the vicissitudes undergone by love and hate impulses; for all of these main groups of phenomena are capable of transference. Needless to add, a theoretical perspective is necessary to prevent undue emphasis being laid on any one factor or set of factors.

But the relation of psychoanalytic theory to the treatment of mental disorders is not confined to examination of psychotherapeutic techniques. Some of the simplest pharmacological devices cry out for psychological investigation. The apparent capriciousness or selectivity of psychic effects of sedatives has long been recorded and the exploitation of these drugs in a series of 'addictions' indicates that they merit psychological as well as pharmacological assessment. A similar problem arises in the case of the 'tranquillizers' with which the psychiatric market is now flooded. Stress, tension, agitation, anxiety, guilt, depression, are current psychological terms; and, to take one example, the relations of anxiety to guilt are extremely close; they sometimes merge imperceptibly, sometimes one masks the other. And both can give rise to depressive affect. The problem then arises what exactly is tranquilized, the instinct tension or the reactive effects of instinctual tension. If the latter, is a purely endopsychic conflict-tension such as guilt more refractory than anxiety readiness? I do not pretend to offer any answer: perhaps

it could be found by a parallel series of experiments on the anxiety states, the obsessional or guilt neuroses and the melancholias or guilt psychoses. I merely wish to emphasize the uses of psychological theory in neuro-pharmacological investigations.

A moment ago I spoke of *perspective* in psychic affairs or, in other words, the balanced assessment of factors, phrases which in the last resort amount to good theoretical and clinical judgement. It is with this concept of perspective that I would like to approach what I consider to be the most signal benefit of Freud's theory of mind, viz. its uses in promoting and regulating *research*. To illustrate this view I have tried to produce in rough outline a research schema, using as paradigm of a research problem the nature of reality sense, admittedly an ambitious subject which calls for at least twenty years of research leisure (Fig. 1). Briefly this schema is based on the assumption that no matter how wide or restricted the scope of the problem (it may be a pan-psychosis, a ramifying character disorder, an isolated symptom construction, a fragment of a recurrent dream or even a symptomatic action) we should seek to approach it, as Freud would have said, metapsychologically, that is to say, applying in series dynamic, structural and economic criteria, the number of which is determined by developmental considerations. Let us assume for the sake of argument that we have assembled the appropriate data: although it may be noted in passing that where selection of data is necessary, this can be best effected with the help of a theoretical scale of values. For the key to success in research does not lie simply in collecting data and studying them, but in knowing where to look for what one wishes to find, and in refraining from dismissing unexamined data which, from the conscious rational point of view, are apparently unimportant. As a preliminary to detailed investigation it is usually convenient to distinguish two main groups of observation, viz. (a) the autoplasmic, in which stress or conflict leads to alteration of the mind or body of the individual, and (b) alloplastic, in which the individual

seeks to modify external conditions, in particular the objects of his instincts.

One can of course start anywhere in the research cycle. When in doubt it is convenient to consider whether the phenomena under consideration are mainly dynamic, structural

reality testing varies at each phase of ego development in accordance with the instincts and mechanisms that exert primacy during each phase.

After that the sequence of research is relatively easy to determine. Thus we proceed to

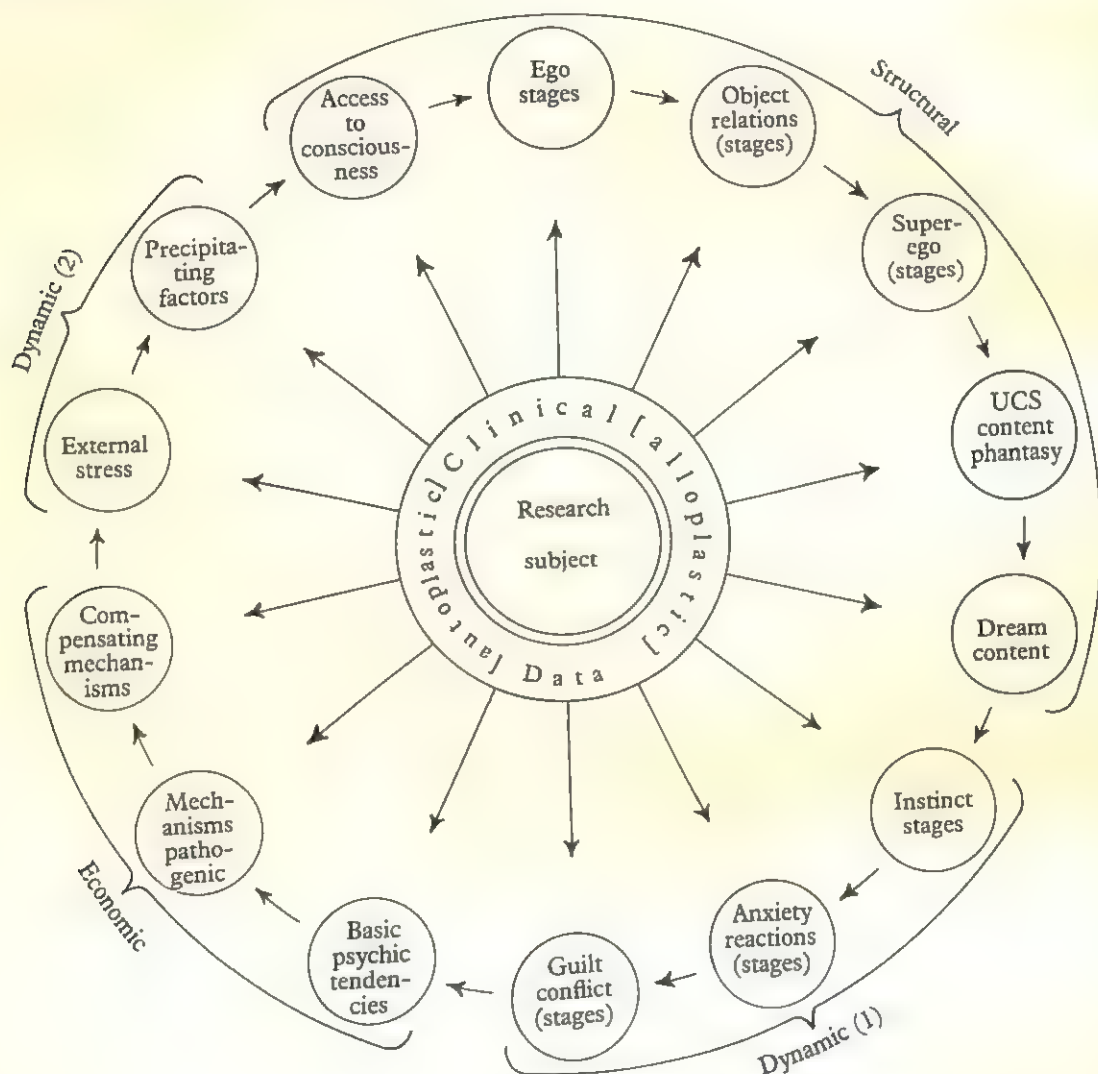


Fig. 1

or economic, and choose a starting point accordingly. In the long run it makes little difference, for these three captions represent merely aspects of a total mental activity. Thus in the case of research on reality sense, it is convenient to start with the *ego* because it is the instrument of reality testing, and because

consider *object-relations*, because in essence reality testing depends on the capacity of the ego to retain contact with the objects of instincts and these vary with each major stage of development.

Next comes *super-ego* investigation, because the super-ego is in closest contact with the

id; because it varies in every stage of development and provides a direct series of imprints extending from unconscious to conscious and layers; finally because it links the individual and social aspects of reality testing.

Then comes the variety of *unconscious content*: because it gives a clue to the nature of the instincts involved and to the stages of ego-object-relationships that have retained sufficient cathexis (or charge) to affect reality estimations.

Dreams may be considered at this stage: because they open up the analysis of structural and dynamic conflicts, and because dream mechanisms operate constantly behind the reality ego and affects its estimations of objective reality.

From these mainly structural approaches we proceed to the first set of dynamic considerations and in the first instance to the study of the *instincts* involved, in particular the libido and the aggressive instincts: because owing to certain qualitative and quantitative considerations, these have the power of abrogating reality sense absolutely, as is seen, e.g., in the narcissistic regression of megalomania.

Of the many dynamic reactions to, or emotional expressions of, instinct, it is convenient to single out in order the direct manifestations of *anxiety* and *guilt*, or, as the case may be, the manifestations of defence against anxiety and guilt, as illustrated in hysteria, the obsessional neuroses and melancholia.

From these dynamic manifestations we then proceed to examine the economic organization of the mental apparatus, starting first with certain *basic tendencies*, the pleasure principle, the repetition-compulsion and the mechanism of regression. For I think the time has come to promote regression from the status of an organized defence mechanism to that of a primary tendency of the mental apparatus. Given sufficient strength, each of these tendencies is capable of modifying or even abrogating the reality principle.

Then comes the examination of *organized mechanisms* from introjection and projection to repression. Indeed one might well write the

word repression across the face of the schema for not only is it capable of modifying reality sense at all points but it is the unknown quantity which can vitiate the results of all mental researches.

Following this examination of mechanisms that contribute to or shape pathogenic processes, comes the investigation of *balancing or compensatory mechanisms* such as sublimation which serve to support and extend reality processes. And from this it is a short step to examine the second set of dynamic factors; viz. the nature of *external stresses* in general, following this with a close investigation of *precipitating stresses*, for without stresses there would be no adaptation to reality either normal or disordered.

Finally, and in all cases, we have to examine, the relation of the foregoing data to the system *perceptual consciousness*. For not only do dreams and phantasies seep into consciousness to the detriment of reality estimations, but under certain circumstances unconscious structural units, particularly those deriving from the super-ego can, as it were, occupy the approaches to consciousness, pressing into their service the mechanisms they are accustomed to employ. And with this investigation we reach once more our starting point—the examination of the ego and its unconscious components.

Needless to say this outline of research is purely schematic. Space and time forbid any detailed subdivision of any of the factorial heads. For example, I have not mentioned inhibition under the heading of defensive mechanisms; or frustration—although it is implied in the dynamic headings; or again, the ‘need for punishment’ although this can be bracketed with instinctual factors on the one hand and guilt factors on the other. Indeed there is scarcely one of the main factor headings which could not be linked to all the others. To speak of the ‘approaches to consciousness’, for example, raises not only structural problems but obviously the significance of the economic mechanism of repression, which, as I have said, is the main stumbling block to

research. Or again the factor of guilt touches on the developmental structure of the super-ego; it must be correlated also with the history of object relations and with instinctual development, in particular with masochism; it overlaps with anxiety and depression; it involves both repetition and regression; it depends on the mechanisms of introjection and identification; it can be affected by external stresses and traumata; following in the track of super-ego inflations, it can flood consciousness; finally it can be transmuted and extend to the external world in the form of persecuted anger of social reform.

I am aware that at first hearing all this must sound extremely complicated: yet it amounts only to saying that psychic phenomena can be approached in a number of ways of which I have indicated only the most essential. The point I wish to stress here is that no research can be regarded as complete in which these fourteen approaches are not fully exploited. Even if in many instances they may draw blank, they should not be omitted. A useful parallel is afforded by systems of organic diagnosis. No physical examination is regarded as complete that does not survey all the body systems. The simplest example is of course the examination of specimens of urine. In the vast majority of cases the results are negative. Nevertheless, the routine cannot be omitted. In point of fact mental examinations involve fewer complications than physical examinations, in the sense that our existing knowledge of most mental processes is still rudimentary. The principles of research are not, however, affected by this circumstance.

At the beginning of this review I remarked that if you are prepared to approach psychiatry armed with a theory of mind, you must decide what variety of theory you are going to exploit. And although I have not concealed my profound belief in the uses and values of Freudian theory I cannot expect to resolve your dilemma with *ex parte* affirmations. In this connexion I can only reiterate that a satisfactory psychic

theory must keep in close touch with the most important aspects of mental life, amongst which I have emphasized the factor of unconscious conflict. To take for example the position of the familiar 'schools' of clinical psychology: although I do not accept and have frequently criticized the theories of Jung, there is this to be said for them that, granted his premises, they do at least offer a fairly consistent theory of mind and moreover one which has some regard for the exigencies of conflict. In that sense it is at any rate near to the facts of life. The decision must of course be left to your predilection, which means I suppose that you will in the long run embrace the theory that affords you the maximum professional peace of mind. And I can only suggest that, in examining this subjective state, you should pay at least as much attention to the satisfactions that can be obtained from clear psycho-biological thinking as to those that arise from therapeutic urges.

At the same time one cannot ignore the fact that in recent years a great change has come over psychiatry; what might be called a hunger for theory has manifested itself. If you read the British psychiatric text-books of thirty-five years ago you will find that, with the exception of the work of Stoddart, few of them manifested any interest in comprehensive theories of mind. At the beginning of the century psychiatry had no theories of mind. Whether the change is due to the alliance between psychiatry and general psychology (which alliance for all practical purposes dates from early in the Second World War), or whether it is due to the fact that it is hard to immerse oneself in modern clinical psychology without developing one theory or other, it seems that we are about to witness the development of what might be called 'team-work theories'. Perhaps the most familiar is that associated with work of Eysenck and the Maudsley school on 'dimensions of personality'. This appears to be derived from two methodological sources, viz. psychiatric observation, including sometimes neuro-psychiatric methods, and the employment under statistical control of laboratory techniques of personality

'testing'. It also combines three theories, viz. a Pavlovian theory of function, a part of Jung's theory of analytical psychology and a personality theory based on the concept of a 'continuum' between normal and abnormal which gives rise to such constructs as 'neuroticism', and 'psychoticism'. To those no doubt in course of time will be added a delinquency continuum and other clinical or socio-psychological captions. For in the long run, and as Eysenck himself has evidently found, one cannot enter a psychiatric province without accepting clinical standards of classification.

For the moment it is not my intention to enter into critical examination of such a 'combined' theory. Indeed one cannot but welcome the advent of any theory in a field in which so few theories have survived. And no doubt the alliance of psychiatry, laboratory psychology and social psychology will result in an extensive array of contributions. I would, however, with the utmost seriousness suggest that under these changed circumstances the psychiatrist should look to his rights if not to his laurels. In other words, he should preserve the authority that comes to him by virtue of his contact with psychic suffering. In the first place he should retain control of any investigation which concerns psychiatric data, however indirectly. Secondly, he should insist not simply that the team-psychologist should produce his training credentials (that goes without saying) but that he should produce his theory of mind. Finally, he should reserve the right (a) to select the clinical material most appropriate to his subject, (b) to interpret *all* the results obtained by the team, (c) having interpreted these results to interpret the conclusions arising from them.

And this brings me at last to the *difficulties* you have to face once you have adopted a theory of mind. Interpretation is the Achilles heel of all psychological research. Yet without interpretation research is reduced to the correlation of descriptive data (which are of course

in the long run, introspections). Psychoanalysis has been and still is criticized for many alleged scientific errors of omission and commission. It has generalized from the study of a few particulars, it has not yet accepted statistical control; it has applied the criteria of abnormal psychology to normal psychology; it has reconstructed stages of mental development which, in the case of early childhood, it is impossible to verify by direct analysis; it has adopted apparently arbitrary systems of symbol translation; it has drawn conclusions from second-hand data. All this can be agreed. But what chiefly renders it suspect in the minds of many is the right it has claimed to interpret both its data and its correlations of data. Even sympathetic observers demand, and demand rightly, that psychoanalysis should produce reliable criteria of interpretation.

Clearly the results of the therapeutic application of analytical theories can prove nothing about them save that the concept of transference seems to have universal validity. As for the 'summation of probabilities' derived from many corroborative sources, although I personally think that this kind of evidence is impressive, I do not believe that it does much more than strengthen the convictions of the convinced. On the other hand, correction does not lie in collecting mass material and submitting it to statistical control, desirable as that is, whenever possible. Owing to lack of definition of statistical units it is, in the case of psychoanalysis, so far only rarely possible. In any case descriptive data, however assembled and examined, remain descriptive. And, as I have indicated, the reconstruction of the earliest stages of mental development will never be capable of direct proof in the analytical sense of the term. It can only be based on and sparingly expressed in working concepts that do not offend against psychobiological probability. Most certainly we should take steps to reduce subjective error as far as that is possible. But as you know, the analytical method of doing so, namely the training analysis, is open to the criticism of indoctrination.

And at this point we would seem to have reached an impasse. On the one hand, it can in my opinion be said that interpretation even more than intuition or imagination is the supreme instrument of psychological investigation, that without interpretation there is no psychology worthy of the name: on the other hand, it can be held that interpretation leaves the door wide open to error. Personally I do not think this impasse can be got over, as some psycho-analysts appear to believe, by esoteric claims that in the country of the blind the one-eyed man is king. On the contrary, let us by all means admit every possible source of error. Let us, however, also admit that in the country of the unconscious it is essential, if not to speak,

at least to attempt to interpret the language that is current there. It may be that one day the reading of the hieroglyphics of the unconscious will become a near-to-exact science. In the meantime, we need not be discouraged by possible sources of error however manifold these may be. The correction of error is in the long run one of the most constructive aspects of research work. And in the last resort, however much we suspect the efficiency of technical means devised to eliminate subjective bias, we can always fall back on the ultimate safeguards of an inexact science, namely, clear psychological thinking, integrity of judgement and an indefatigable concern with the clinical data of observation.

A HISTORY OF GROUP AND ADMINISTRATIVE THERAPY IN GREAT BRITAIN

F. KRÄUPL TAYLOR*

During the Second World War some British psychoanalysts in the Forces became interested in the therapeutic potentialities of group processes. Their interest was aroused by the realization that many soldiers became psychiatric casualties because they had been put into the wrong jobs. To insure a more adequate placement of Forces personnel a new selection machinery was evolved (Sutherland & Fitzpatrick, 1946).

New methods of choosing suitable candidates for training as officers were also introduced by the War Office Selection Boards. These procedures made use of certain situational group tests and, in particular, of the 'leaderless group' test that had been suggested by Bion (1946).

This test derived its rationale from the Freudian theory of group psychology. According to this theory healthy groups need an aristocratic structure which divides the membership into leaders and followers. Thus, if it is one's task to discover potential leaders, one might obtain helpful clues from the behaviour of candidates in a group situation. A practical task is set to a group of candidates and the way in which they go about solving it is observed.

Such a group is unstructured and 'leaderless' at first. Everyone starts from a base-line of equal group status. But as the candidates proceed to tackle their allotted task, a hierarchy of status will develop. Some men will assume leader roles, whereas others will have to choose between obstructing the leader or collaborating with him.

In 1943, Bion & Rickman were sent to the Northfield Military Hospital to deal with un-

ruly conditions that had developed in its Training Wing. Rickman started a series of group discussions with the men, but Bion set himself a more ambitious task. He aimed at transforming the traditional authoritarian structure of a military hospital. He argued that there was a bad group spirit among the men who were dissatisfied with Army discipline and wanted to return to civilian life. To improve their group spirit, they had to be converted into a responsible co-operative community so that a feeling of belonging and a healthy group structure could develop. To achieve this, Bion took the unprecedented step of relinquishing the authoritarian role with which military and hospital tradition had endowed him. The patients had to choose between suffering the discomforts of a chaotically unstructured community life or shouldering the responsibility of organizing communal activities themselves. They could no longer blame the Army when things went wrong. They had to deal now with their own antisocial and disruptive tendencies.

Bion stayed at the Northfield Military Hospital only six weeks. He temporarily changed a troublesome community into an organization that functioned adequately. His ideas, however, bore fruit later in the second 'Northfield Experiment' when his administrative innovations were worked out for a whole hospital and were linked with methods of small-group psychotherapy (Main, 1946; Bridger, 1946; Dewar, 1946; Foulkes, 1946a-c, 1948).

The development of therapeutic group activities in Great Britain since the war can be largely traced back to the two 'Northfield Experiments'. Many aspects were carried on independently. The subsequent development will therefore be reviewed under the following headings: (1) Group Selection Procedures; (2) Administrative Therapy; (3) Therapeutic

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Communities; (4) Small Therapeutic Groups; (5) Research.

It must, however, be mentioned that some therapeutic group activities had arisen prior to, and independently of, the 'Northfield Experiments'. Bierer (1940, 1942, 1944*a, b*; see also Bierer & Haldane, 1941) had started group treatment in mental hospitals by means of therapeutic social clubs and small discussion groups in 1938. Snowden (1940) had used a lecture-discussion method in treating civilian outpatients. Blair (1943) had employed a similar method with neurotic soldiers. Maxwell Jones (1942, 1944, 1952) had transformed his Effort Syndrome Unit at Mill Hill Emergency Hospital into a therapeutic community in 1942 by means of lecture-discussions, socio-dramatic performances, and by using nurses as therapeutic aides in educating his neurotic soldier-patients towards an acceptance of group responsibility. Foulkes & Lewis (1944) had treated civilian outpatients with an analytic form of group treatment in 1941-42. Ling (1944, 1946; see also Ling, Purser & Rees, 1950) had introduced a form of community therapy in the Roffey Park Rehabilitation Centre when it was founded by the Council for the Rehabilitation of Industrial Workers in 1943.

GROUP SELECTION PROCEDURES

The employment of group tests in the selection of officer candidates was one of the most original features of War Office Selection Boards in the Second World War. Since 1942, officer candidates who attended these Boards were observed in a number of group situations such as informal social gatherings, formal group activities, and 'leaderless group' tasks. Harris (1949) has given a full account of the group tests employed. Other tests, as well as assessments by interview, were also taken into account in the selection programme.

Since the war there has been a growing tendency to use a similar battery of test procedures in the selection of candidates for certain high-grade appointments. Such a scheme was, for example, introduced by the

Civil Service Commissioners who are responsible for the selection of candidates for central government posts. Civil Service Selection Boards were set up which observed and evaluated candidates in a country house near London during week-ends for 48 hours, using, among other tests, group discussions of problems which might be met with in Government work, and other 'life-like' group situations.

Industry did not lag behind. Bridger (1947) reported at the 24th Annual Conference of the Industrial Welfare Society that group tests for the selection of junior management staff were employed by certain firms, such as Unilever.

Fraser (1946, 1947, 1949, 1950) published several papers describing this novel type of selection for executive positions in industry. Further relevant papers were published by Higham (1952) and Wilson (1954). Beverstock (1949) applied group methods in the selection of youth group leaders; and Herbert (1951) used them to select candidates for teacher's training.

Universities began to experiment with group selection procedures in order to find the most promising students among a surplus of applicants. Johnson (1952) and Aitken and Johnson (1952) described the use of group methods in the selection of medical students at University College, London. Warburton (1952) gave an account of the use of these selection methods in the Departments of Education and Dentistry at Manchester University.

There can therefore be little doubt that group selection procedures had attracted attention in many quarters. There was opposition as well, of course. The Civil Service Selection Boards, for instance, came under fire in both Houses of Parliament, but much of the criticism levelled against them proved to be unjustified.

Many of the authors mentioned above have stated their conviction that group selection procedures have proved their value. Yet there have been few investigations of their actual success. Morris (1949) reported on the results of officer selection in the British Army during 1942-5 when group tests had been in use. He found that candidates selected then did signifi-

cantly better when sent to Officer Cadets Training Units than candidates who had been passed before the introduction of group tests at the Selection Boards. But there was very little difference between the two classes of candidates when their abilities were rated under conditions of actual warfare. Vernon & Parry (1949), in a careful and exhaustive study of personnel selection in the British Forces, came to the conclusion that the value of the new-type War Office Selection Boards had not been adequately established. The undoubted popularity of these Boards was due to the fact that the life-like group tests had caught the imagination, and won the confidence, of many people in the Forces. It certainly had the desired effect of stimulating the recruitment of officer candidates at a time when the British Forces were short of officers.

In a follow-up investigation of the work of the Civil Service Selection Boards, Wilson (1948) found that the success of group selection procedures was not spectacular and was surpassed by the success of certain cognitive tests. Vernon (1950) arrived at similar conclusions.

The assessment of prospective medical students by means of group interviews was found to be of no help in predicting which students would pass their second M.B. examinations. The correlation between assessment through group interview and examination success was -0.125 (Aitken & Johnson, 1952).

The only promising result of group tests was reported by Beverstock (1949) who found, in a follow-up after one year, that group techniques in the selection of youth leaders yielded results that had a correlation of $+0.57$ with success in the job.

Yet in spite of these rather disappointing results of investigations into the predictive power of group selection tests, there is no doubt that the tests had been popular with candidates and examiners alike. It may be that the realism of the socio-dramatic test situation caused candidates and examiners to believe that the selection would be fair. Even if this belief has turned out to be largely illusory, the

group tests were nevertheless of value, especially during the war. It must be admitted, however, that they were perhaps therapeutic rather than diagnostic, because their chief effect seems to have been to reduce the anxieties and uncertainties of candidates.

ADMINISTRATIVE THERAPY

The second 'Northfield Experiment' revealed the necessity of re-examining the administrative structure of our mental hospitals. Since the latter part of the nineteenth century mental hospitals had grown in size and developed into prison-like institutions whose main task was not so much the treatment of their inmates as their segregation from the outside world.

This chiefly custodial outlook began to change in the thirties of this century with the introduction of occupational treatment and shock therapy. Asylums were converted into hospitals, though the custodial atmosphere lingered on in chronic wards. The administrative structure of mental hospitals remained everywhere rigidly authoritarian.

It was not until the Second World War that demands for a more permissive and less paternalistic regime began to be heeded by an occasional medical superintendent who started to encourage some of his patients to accept more self-determination and responsibility for their activities in hospital.

T. P. Rees, at Warlingham Park Hospital, opened the doors of all but two of his twenty-three wards and instituted a policy which fostered social contact among his patients (1954, 1957; see also Rees & Glatt, 1955; Sandison & Chance, 1948; Chance, 1948; Mitchell & Zanker, 1948; Sandison, 1951; Branden & Zanker, 1955; Glatt, Weeks & Whitely, 1957). Rees also contributed largely to the Third Report of the World Health Organization's Expert Committee on Mental Health (1953).

May (1956) reported that, at Netherne Hospital, there had been no open wards or villas in 1932, and only 5 % of the patients had been on parole within the boundaries of the estate. In

1956, twenty-two out of thirty-seven wards and villas were open, and 60 % of the patients were on parole.

The unlocked door became the symbol of progress in many mental hospitals. In 1949, Bell (1955) opened all the doors of Dingleton Hospital, Melrose. MacMillan (1954) at the Mapperly Hospital, Nottingham; Stern (1957) at the Central Hospital, Warwick; and Mandelbrote (1958) at Coney Hill, Gloucester, followed suit. But Gibson (1954), Good (1954) and Whiteley (1958) were able to draw attention to the fact that the value of open doors in mental hospitals had been known many years before. They referred to publications by B. Tuke (1876, 1881) concerning Fife and Kinross Asylum and three other Scottish mental hospitals, to T. S. Good (1930) concerning Littlemore Hospital, Oxford, and to D. H. Tuke's *History of the Insane in the British Isles* which appeared in 1882.

But critical voices were also heard before long. They warned against extreme measures and suggested that some locked wards are indispensable in mental hospitals. Bickford (1958), in particular, cautioned against accepting the slogan of the open door as a substitute for proper treatment.

Increased liberty and self-determination were not the only characteristics of the new administrative spirit in mental hospitals. Emphasis began to be placed on appropriate incentives to work. Routine activities in occupational-therapy departments were no longer considered good enough. In any case, chronic patients had all too often remained without occupations as they appeared too deteriorated for employment. Bickford (1954, 1955a, b) entered a plea for the forgotten patients in chronic wards, and described the results he had achieved at De La Pole Hospital by encouraging the nurses to take a special interest in promoting activities, games and discussions for long-stay patients in their charge. The incentive to work obviously did not come from a desire to be active, nor from any appeal of the work itself or pride in achievement. It resulted from improved nurse-patient relationships as

Cameron, Laing & McGhie (1955) and Freeman, Cameron & McGhie (1958) pointed out when reporting their success with a specially encouraged group of schizophrenics at the Glasgow Royal Mental Hospital. Similar accounts came from other hospitals, Warlingham Park (Rees & Glatt, 1955), Netherne (Bennett & Robertson, 1955; May, 1956a, b; Folkard, 1956, 1957), St Ebba's (Merry, 1956), Fulbourn and Addenbrooke's (Clark, 1956; Houston, 1956), Runwell (Robin, 1957) and Glengall (Sherret, 1958).

Baker & Freudenberg (1957) speculated whether a flagging of interest on the part of nurses in chronic wards and a consequent relapse into indifferent custodial methods of care could be prevented by instituting regular changes in the nurses' working schedules and duties. Martin (1955) blamed authoritarian attitudes among hospital staff as responsible for the conversion of chronic patients into docile 'institutionalized' cogs within the administration's wheel and he pleaded for the establishment of real human relationships between hospital staff and patients.

For less deteriorated patients gainful employment has proved one of the most powerful incentives to work and therefore one of the best means of aiding patients to retain contact with reality. At a time of full employment and shortage of workers it has not been too difficult to persuade employers outside the hospital to offer work to suitable patients, singly or in groups. The earning of money by patients posed, however, some clerical problems at first (Miller, 1957; Bickford, 1957; McCowan, 1957).

A new venture was the establishment of an industrial workshop at Banstead Mental Hospital. The patients did not have to leave the hospital to earn wages. The money incentive was strong enough to enable even chronic patients to carry out comparatively unskilled tasks at the work level of normal people (Carstairs, O'Connor & Rawnsley, 1956; Baker, 1956). At Cheadle Royal an experiment has been begun which allows patients to be employed alongside normal workers from

outside in a factory workshop within the hospital grounds.

Not only chronic psychotics, but also adult imbeciles have been shown to be sufficiently trainable so that they can be made to hold their own in collaboration with high-grade defectives (Loos & Tizard, 1955; Clarke & Hermelin, 1955; O'Connor & Tizard, 1956; O'Connor, 1957). A liberal employment policy aiming at resettling feeble-minded patients in the community has also been pursued by some mental defective hospitals (e.g. Hilliard, 1954, 1956).

The employment of psychiatric patients, whether gainfully or not, and the greater freedom granted to them by hospital authorities cannot however be regarded as the most essential elements of successful administrative therapy. What matters most are, by general consent, the interpersonal relations between the people working in a hospital; the relations between superintendent and medical staff, between medical staff and nurses, between nurses and patients, and between the patients themselves. This fact was stressed in the World Health Organization's Third Report of the Expert Committee on Mental Health (1953), and it has been re-emphasized by all the writers on the subject (Rees, 1954, 1957; Maxwell Jones & Rapoport, 1955; Sawle Thomas, 1956; Clark & Hoy, 1957; Tetlow, 1957). As Clark (1958) remarked: 'As he [the superintendent] treats his staff so will they treat the patients.'

The importance of administrative therapy has been recognized by the King Edward's Hospital Fund which, in 1957, initiated special study courses in administration for medical superintendents. The Fund had previously established residential staff colleges in London for non-medical hospital administrators, matrons and ward sisters.

The importance of congenial interpersonal relations for the efficiency of organizations has also been acknowledged by many industrial concerns which, in post-war years, have increasingly utilized the services of consultants in personal relationships. The Tavistock Institute of Human Relations have been pioneers in this

field. Their consultants have carried out therapeutic work under the name of 'action research' in industrial firms who had appealed for their help. The consultants acted as therapists in executive and consultative committee meetings. Their object was to unearth and correct disturbing emotional undercurrents in interpersonal relations. They also organized special discussion groups which they guided from a therapeutic angle (Jaques, 1947). The work carried out by the Tavistock Institute of Human Relations during 1948-50 at the Glacier Metal Company, a light engineering factory with about 1500 employees, has been described in a series of books and papers (Jaques, 1950, 1951; Rice, Hill & Trist, 1950; Rice, 1951*a, b*, 1952; Hill, 1951; Jaques, Rice & Hill, 1951; Rice & Trist, 1952). Trist & Bamforth (1951) examined the social and group-psychological effects of the introduction of the longwall method of coal-getting into British coal mines. Jaques (1956*a, b*) came to the conclusion that not only neurotic trends but also unconscious psychotic phantasies of the kind postulated by Melanie Klein (1950) interfered with the normal working of social relations in industry.

THERAPEUTIC COMMUNITIES

All hospitals are, or should be, therapeutic communities which are administered in a benevolent spirit for the sake of the patients rather than for that of a bureaucratic order. However, the term therapeutic community has been increasingly used recently to denote organizations in which permissive interpersonal and group relations are used as the main remedial agents. Whereas the purpose of administrative therapy is to provide a social atmosphere in a hospital in which other kinds of therapeutic measures have the best chance to succeed, in therapeutic communities it is the social atmosphere itself that is regarded as the main therapeutic measure.

Northfield Military Hospital may have been the first therapeutic community of this sort. Shortly after the war when many repatriated

prisoners of war encountered difficulties in civilian life, twenty Civil Resettlement Units were established and administered as therapeutic communities. The atmosphere in them was calculated to encourage the men to assume their share of responsibility in organizing the life of their Unit—a responsibility which had not been theirs in the Forces and prisoner-of-war camps. Wilson, Doyle & Kelnar (1947) described the group techniques employed to achieve this purpose. So did Maxwell Jones (1946, 1952) who made use of specially trained nurses in his Unit and introduced the technique of socio-drama.

Curle (1947) and Curle & Trist (1947) attempted to assess the success of these Units by means of a follow-up study. They showed that repatriated prisoners of war who attended Civil Resettlement Units had met with significantly more stress on their return to civilian life than those who did not attend. They may have achieved, however, a significantly better degree of ultimate adjustment to civilian life than their comrades who had not availed themselves of the therapeutic opportunities offered in Civil Resettlement Units.

A brief, but inconclusive, observation on the effect of a therapeutic community on a group of six 3-year old children who had survived German concentration camps was reported by Freud & Dann (1951; see also Freud, 1955). The children were accommodated in a country house near London for a while. It took them weeks before they could respond in some measure to the kindly and affectionate spirit with which they were treated. They did not seem to form close emotional ties with the persons who looked after them, but had close group ties among themselves.

Some therapeutic communities have tried to remove—with more or less success—the role barriers that usually separate patients from nurses and doctors, and to hand over to the patients the responsibility for the formulation of rules and regulations governing hospital life.

The Cassel Hospital under Main (1946) went furthest, perhaps, in granting self-determination to patients, even to the point of risking chaos

and anarchy to compel the patients to tackle community problems themselves.

The Roffey Park Rehabilitation Centre under Ling (1944, 1946) specialized in the community treatment of maladjusted industrial workers. Follow-up studies by Ling, Zausmer & Hope (1952) and by Ling, Wilson & Briggs (1955) showed that 68 % of the patients responded well as judged by the employing firms 2–7 years later, though only 52 % of the patients admitted improvement.

The Social Rehabilitation Unit at Belmont Hospital under Maxwell Jones (1948, 1949, 1952, 1954*a, b*, 1957) set itself the difficult task of treating psychopathic patients who had a poor employment record. In this Unit specially trained social therapists took the place of nurses. The therapeutic programme and the rationale for it has been described in many publications (Freeman, 1952; Pomryn, 1952; Baker, 1952; Baker, Maxwell Jones, Merry & Pomryn, 1953; Merry, 1953*a, b*; Maxwell Jones & Rapoport, 1955). The Unit has been built up on an integrated system of group activities which include psycho- and socio-drama, as well as group discussions of the policy and administration of the Unit. A follow-up inquiry of the results achieved was carried out by Tuxford (1952). But her inquiry was inconclusive because a control group of unemployable psychopaths, who had never been sent for treatment to the Unit, turned out to consist of more severely disturbed individuals than those who received treatment. Her data were analysed by Sandler (1952*a*) who estimated that 67 % of the treated patients had made a fair to very good adjustment 6 months after discharge. Sandler (1952*a, b*) found a high correlation between improvement in hospital and subsequent adjustment to work. Rapoport & Rapoport (1957) remark that the principles on which therapeutic communities function break down in emergencies. The medical staff do not delegate responsibility to the patients unconditionally. There is a breaking point when they assert their authority and lay down the law in much the same way as is done by the administrators in

other hospitals with a much lower level of tolerance and permissiveness. Rapoport (1956) and Rapoport & Skellern (1957) point out that the permissiveness of therapeutic communities leads to recurrent episodes of social disorganization which cause personal and interpersonal disturbances. The non-punitive resolution of such disturbances by means of rational analysis and group discussion can be therapeutic. But some patients respond adversely to the tension and anxiety generated at times of social disorganization.

Some experimental therapeutic communities have been established within the confines of mental hospitals. At Warlingham Park such a unit was organized for ten male and ten female neurotic patients. They were granted a maximum of self-government, though they had to accept rules made by the medical superintendent which applied to the total hospital. Their stay at the unit was limited to two months (Martin, Glatt & Weeks, 1954; Glatt, Weeks & Whiteley, 1957). Another small unit at the same hospital specialized in the treatment of alcoholics (Glatt, 1955). In the group discussions of this unit, spiritual topics occurred so frequently that the hospital chaplain was invited to become a regular visitor.

Attempts have also been made to introduce some degree of self-government and permissiveness into the residential treatment of approved schools and Borstal Institutions. At the Red Hill School, Maidstone, Kent, to which only boys of superior intelligence are admitted, some self-government by elected committees with a minimum of interference by adults was introduced. There is also a Court that sits twice weekly and is composed of boys specially elected as Bench members. Adults only rarely advise the Court, which can impose small fines on offenders (Bloom, 1956). A comparison made by Jones (1958) between two approved schools with permissive regimes and two approved schools of the customary disciplinary kind yielded the finding that a permissive regime is more likely to encourage positive attitudes towards adults in the maladjusted children of the schools. In the open Borstal for

girls at East Sutton an attempt has been made to grant some self-government to the occupants with a minimum of direction and supervision by the staff, and similar attempts seem to have been made in some other Borstals (Fox, 1952).

The applicability of the permissive principles of therapeutic communities to groups of delinquent or 'unclubbable' adolescents was also explored. Peter Scott (1956) found that adolescent street groups in London are loose shifting associations. Well structured groups, which had been reported as common in America, are rare over here. An experiment to provide difficult adolescents, who were 'unclubbable' by the standards of orthodox youth organizations, with a permissive and democratic club was carried out in the East End of London on board a barge that had been specially acquired for the purpose. This 'barge experiment' proved a partial success (Spencer, 1950; Turner, 1953).

Social clubs and day hospitals can be run as therapeutic communities and in this form they have gained wide popularity recently.

Bierer (1940) established the first therapeutic social club at Runwell Mental Hospital in 1938. This was a club for in-patients. It was soon followed by clubs catering for out-patients. Bierer's Institute of Social Psychiatry now administers several clubs in different parts of London. In his opinion (1940, 1942, 1944*a, b*, 1948, 1949), these clubs provide 'situational treatment' by enabling patients to find an improved adjustment to social situations. Social clubs have to-day been established in many places and a fairly large number of accounts of them have appeared in the literature (Blair, 1949; Thompson, 1949, 1955; Brody, 1949; Aron, 1949; Butcher, 1949; Kras, 1957). Senft (1949) examined the hierarchical structures of these clubs and the tensions generated in them. Peter Scott (1949) warned against sending patients to social clubs prematurely. Taylar, Stickland & Lindsay (1949) described a club for mental defectives; Branden & Zanker (1955) a club for geriatric in-patients. Chance (1948) and Langham (1956) considered the role of the psychiatric social worker in these clubs. Morgan & Tylden (1957) reported the

founding of the 'Stepping Stones' social club by the 'Friends of Bromley Hospital'.

The first day hospital in Britain was opened by Bierer (1951) at Marlborough Place, London, in 1948. Its aim was to offer the remedial facilities of a therapeutic community without removing the patient completely from his home environment. Aron & Smith (1953) and Smith & Cross (1957) reported on the day hospital in Bristol; Harris (1954, 1956) on the day hospital at the Maudsley Hospital in London; Cosin (1955*a, b*) on the day hospital in his geriatric unit at Oxford.

SMALL THERAPEUTIC GROUPS

At the Northfield Military Hospital small-group psychotherapy was widely used. Since then it has become an integral part of the treatment programme in many therapeutic communities. It has also become popular with many psychiatrists who see in it a possible means of narrowing the gap between the large number of patients needing psychotherapeutic help and the small number of psychiatrists who have the inclination, and can find the time, to give psychotherapy. It has therefore been applied to many in- and out-patients in a variety of forms.

The groups generally range in size from six to ten patients, and most of them meet once a week for 1½ hours. The majority of group therapists in Britain have modelled their techniques on psychoanalytic practice, and are permissive and non-directive rather than suggestive and educative.

Permissive forms of group therapy with in-patients may give rise to disturbances that extend beyond the treated group, involve other patients and upset staff members who feel their authority undermined. Klein (1950) indicated some of these difficulties; Taylor (1957*a*) pointed out that the group patients have been chosen from a great many in-patients and come to regard themselves as a specially favoured *élite* with all the disagreeable consequences that may flow from this. However, this *élite* feeling may also be responsible for much of the

clinical improvement shown by group patients in hospital. Yonge & O'Connor (1954) mentioned this possibility when analysing the beneficial effect of group therapy with anti-social mental defectives at Darenth Park Hospital. They considered it possible that the improvement had been largely due to the special attention the treated group members received which distinguished them from other in-patients.

It is important to keep the possibility of a therapeutic group effect of this kind in mind. It has long been known to industrial psychologists as the 'Hawthorne effect', a name which commemorates the experiments carried out by Elton Mayo and his associates at the Hawthorne Works of the Western Electric Company in Chicago between 1924 and 1927 (Mayo, 1933; Roethlisberger & Dickson, 1939). It is interesting to note that the Hawthorne investigators used non-directive methods of interviewing which were similar to the permissive techniques used in group therapy today. Many placebo responses (Tibbetts & Hawkings, 1956; Trouton, 1957) and many reports of successes with new drugs may go back to the workings of the Hawthorne effect. Even therapeutic methods which appear fairly well established to many psychiatrists may have the same, but unrecognized, group-psychological foundation. This may, for instance, be true of the insulin-coma therapy of schizophrenics (Ackner, Harris & Oldham, 1957).

Many group-therapy papers deal with special categories of in-patients; schizophrenics (Kräupl, 1947); adolescent behaviour problems (Warren, 1952; Sohn, 1952; Cameron, 1953); involutional depressives (Freeman & Cameron, 1953; Cameron & Freeman, 1955); mental defectives (Rudolf, 1955; Heller, 1955; Yonge & O'Connor, 1954; O'Connor & Yonge, 1955); geriatric problems (Benaim, 1957).

Mackwood (1950, 1953, 1954) introduced group treatment into prisons. He started it in Wormwood Scrubs Prison in 1946 in a hospital ward of ten beds. This work was continued by Landers, Macphail & Simpson (1954). Even

some aggressive psychopaths were found to benefit from the treatment, though no group could tolerate more than two or three of them (Landers, 1957). Group therapy has also been used in Wakefield Prison (Roper, 1953) and has since been gaining increasing acceptance in prisons (Report of the Commissioners of Prisons for 1956).

Most therapeutic groups, however, do not deal with patients in hospital, but rather with ambulant patients in out-patient clinics and private practice. The treatment of these patients has been most extensively described by Foulkes (1946*a-c*, 1948, 1950, 1951*a, b*, 1952, 1953*a, b*, 1955, 1957; see also Foulkes & Lewis, 1944; Foulkes & Anthony, 1957). Foulkes has also used groups for purposes which are not directly therapeutic. He employs introductory groups which have a diagnostic aim, permitting a number of doctors to observe a number of patients in a group situation. Once patients have been chosen as suitable for group treatment, they are put into preparatory groups and they continue in them until a vacancy occurs in a suitable permanent group (Foulkes & Parkin, 1957).

Special group problems with out-patients have been considered in several papers: treatment of a phobic patient (Kräupl, 1948); of dermatitis (Klein, 1949); of childhood neuroses at different ages (Lewis, 1954, Anthony, 1957); of employment problems (Williams, 1955), and of juvenile delinquents (Perry, 1955). Bowlby (1949) reported on the value of two to three initial therapeutic sessions with the family group of father, mother and child when they first attend a child guidance clinic.

Among special group techniques used by some therapists is Moreno's (1952) psycho- and socio-dramatic technique which encourages patients to act out in the group, or before a larger audience, a personal or social problem either spontaneously or after rehearsals (Maxwell Jones, 1944, 1948, 1949, 1952; Bierer, 1948, 1951; Slowik, 1958), artistic products, such as paintings submitted by the patients for interpretation in the group

(Sowerby, 1949; Sandison, 1951; Martin *et al.*, 1954; Landers, Macphail & Simpson, 1954), and music therapy (Mitchell & Zanker, 1948; Brooking, 1957).

Most group therapists are convinced of the effectiveness of group treatment and have not bothered to ascertain to what extent their conviction is borne out by fact. Foulkes & Anthony (1957), for example, talk of a 'spontaneous' therapeutic process in groups conducted on permissive lines. Such groups, they say, 'without any pressure from him [the group conductor] will prefer to take the therapeutic road, even though it is a little rougher'. There can be no doubt that group treatment is effective in increasing the turnover of patients and is thus a boon to the busy clinician. This is particularly true of so called 'open' groups in which there are small nuclei of faithful attenders and large numbers of other patients who come for a while and then disappear—unimproved and forgotten. In small provincial towns many patients dare not join therapeutic groups or even attend a psychiatric out-patient clinic because their reputations might suffer, if the fact became known (Sandison, 1955).

There are hardly any exact studies of the results of group treatment. Phillipson (1958) analysed the outcome in twenty-one groups treated at the Tavistock Clinic in London. A total of 238 patients had attended these groups. About a quarter discontinued in the first six months. Another quarter or so left before the end of two years. One hundred and twenty-nine patients carried on for more than two years. Seventy-eight per cent of these improved. Thorley & Craske (1950) came to the conclusion that there was no difference between individual and group therapy in hospital patients. Yonge & O'Connor (1954) compared a hospital group of seven antisocial mental defectives after six months of group treatment with a comparable untreated control group and found a significant increase of diligence and a significant decrease of unsatisfactory behaviour in the treated group. Taylor (1950*a*) obtained an improvement in eleven of fourteen patients attending two

separate groups, for ten and seven months respectively. He found a significant correlation of $+0.70$ ($P=0.02$) between a person's popularity status in the group and the degree of ultimate improvement. Patients who were disturbed by group discussions of sexual topics did better than others ($r=+0.63$; $P=0.04$); those who had a neurotic fear of talking (lalophobia) did worse than others ($r=-0.65$; $P=0.04$).

Permissively conducted groups may be used to provide students with the opportunity of experiencing the emotional impact of group processes on themselves. Herbert & Trist (1953) employed the method in didactic seminars of school teachers; Johnson (1950, 1955) and Polani (1955) in the teaching of medical students; Balint (1954) in teaching general practitioners the skills needed for the handling of psychological problems in their patients, and Balint & Balint (1955) in the training of social workers to manage interpersonal relations in the families they had to deal with. Students wishing to acquire the techniques of group treatment have been put into therapeutic training groups where they can learn through personal experience as well as instruction. No papers have as yet appeared describing such training groups. At the Tavistock Clinic, London, Sutherland allows students to observe through a one-way screen his conduct of a therapeutic group. His patients are aware of this concealed observation and do not mind it.

There have been several attempts to supply the practice of group therapy with its own theoretical superstructure. But so far these attempts have succeeded only in offering some interesting speculations and in coining a number of different terms for similar phenomena. Bion (1947, 1948, 1949*a, b*, 1950*a, b*, 1951, 1952), using the 'intuitions developed by present-day psychoanalytic training' and thinking publicly, as it were, in a series of papers, arrived at a division of group processes into two categories: (a) rational, overt and reality-oriented; (b) emotional concealed, phantasy-dominated. Rickman (1950*a, b*) supported some of Bion's early formulations

and characterized his approach as a-historical. Ezriel (1950, 1952*a, b*, 1956) and Sutherland (1952) also emphasize their a-historical 'here-and-now' evaluation of group processes. This attitude is shared by many other group therapists.

The same wide agreement, in spite of differences in detailed terminology, applies to concepts of a division of group processes into overt and hidden elements. Ezriel (1950, 1952*a, b*) and Taylor (1949, 1952) distinguished between the manifest content of discussions and latent group tensions or collective motivations. Foulkes (1948, 1951, 1955) spoke of manifest and latent 'occupations' of groups and their 'collective, social or interpersonal unconscious'. Schindler (1951*a, b*, 1952, 1953) referred to basic family patterns which influence group members to see the group as a mother symbol and the therapist as a father figure. Buck (1950*a, b*, 1951) expressed the opinion that, below the surface of group phenomena, telepathic and other psi-functions of extra-sensory perception are active. The general theoretical orientation of most group therapists is Freudian, Kleinian or Jungian, though there are many eclectics. Foulkes (1948, 1953*a*, 1957; see also Foulkes & Anthony, 1957) has however emphasized repeatedly that one should not apply the same concepts indiscriminately to the psychoanalysis of individuals and the psychotherapy of groups. He has coined the name 'group-analytic psychotherapy' to stress the difference between individual psychoanalysis and his own group techniques and theories. He has suggested a number of new terms to replace those used for analogous processes in individual psychoanalysis.

Some attention has been devoted to certain characteristic group processes which may have wider implications. Foulkes (1948) and Herbert & Trist (1953) discussed the influence of absent members on group proceedings. Taylor & Rey (1953) described an attempted enactment of the scapegoat theme in a therapeutic group. Taylor (1956*a*) observed a brief epidemic of conversion-hysterical symptoms

in a female hospital ward and attempted to analyse it in relation to other mental epidemics that had been reported in the literature (see also Taylor & Hunter, to appear).

RESEARCH

Some papers reporting the results of special investigations have already received attention in the preceding sections. Others still require mentioning.

The Medical Research Council created a Unit under the direction of Lewis (1953) to study problems of occupational and social psychiatry. The findings of this Unit bear out the therapeutic and social importance of administrative procedures which encourage training and self-reliance rather than segregation and custodial care. Gordon (1953); Gordon, O'Connor & Tizard (1954, 1955); O'Connor & Claridge (1955), and Claridge (1956) have investigated the effect of incentives on the work performance of imbeciles. The setting of easily achieved goals, self-competition with previous achievement, and rivalry with other groups of patients proved of high incentive value. The most important and lasting incentives had their origins in feelings of social acceptance and encouragement. Even imbeciles with an average I.Q. of only 34 were sensitive to signs of social approval or rejection. These findings were corroborated by Walton & Begg (1955). Tizard (1953) studied the effect of different types of supervision on the behaviour of mental defectives in a sheltered workshop and came to the general conclusion that the majority of patients behaved well when the supervision was strict or friendly, but that their work suffered under a laissez-faire regime.

Folkard (1956, 1957) studied acts of aggression in a female hospital ward for acutely disturbed psychotic patients. The most aggressive patients were found to be also unco-operative. Aggressive acts were reduced when nurses learned to adopt a more personal attitude to the patients.

O'Connor, Carstairs & Rawnsley (1957) found that patients in closed wards were signifi-

cantly less aware of hospital news than patients in open wards, irrespective of diagnosis, age and length of stay.

There have been several sociometric analyses of groups to ascertain the feelings of group members towards each other and to study subgroupings and clique formations. Rose (1956) found that sociometric tests revealed social groupings in a Borstal Institution better than the housemasters' observations. Croft & Grygier (1956) reported that truants and delinquents tended to have a low sociometric status in a normal classroom, i.e. had little influence on others and were unpopular. Teachers' estimates of unpopularity were more reliable than those of popularity. Sandison & Chance (1948) applied sociometric tests to therapeutic groups and noticed that open groups with a changing membership never developed adequate cohesion.

Taylor (1950*b*, 1951, 1954, 1955, 1956*b*, 1957*b*) developed a sociometric method which takes account, not only of the feelings group members develop towards each other, but also of the feelings they believe to have evoked in others. The method allows the measurement of group phenomena in three dimensions: a public dimension consisting of hierarchical structures such as hierarchies of dominance or popularity; a dyadic dimension indicating the way a person distributes his feelings among his group colleagues, or the distribution of feelings he believes to have aroused in them, and an autistic dimension referring to a person's overall attitude towards the group and his various colleagues, or to his general expectation of their attitudes to him.

These measurements are helpful in studying group phenomena and in conducting group psychotherapy. They reveal the network of interpersonal feelings, allow the assessments of general emotional tendencies, and sometimes permit the therapist to forestall undesired developments. Outsiders and 'isolates' are quickly spotted, so that the therapist can attempt to draw them closer into the group before it is too late. It was found that outsiders in a group dare not reveal what they feel

for others and remain ignorant of the feelings of others for them. In female groups there is a significantly more candid display of general feelings than in male groups. People who are dominant also tend to be popular. The correlation between dominance and popularity is $+0.51$. But there are exceptions and it is advisable to look out for some of them. For example, patients who are very dominant and yet unpopular are inclined to terminate their group treatment prematurely on some plausible pretext.

A helpful device was introduced by Foulkes (1953*b*)—a chart to keep a record of attendance and of changes in group membership.

The hypothesis that groups composed of members of the same sex stimulate homosexual fears was tested experimentally (Taylor, 1949). Two therapeutic groups of long standing, one consisting of men and the other of women, were amalgamated so that they formed two groups with both sexes represented in each. The result of the experiment supported the hypothesis. The experiment also threw some light on initiation ceremonies in therapeutic groups, which generally consisted in submission to the demand of making a clean breast of one's symptoms and personal difficulties.

Talland (1954*b*) showed that dominant group members do not owe their status to an intuitive grasp of group opinions before these had been freely discussed. They influenced and fashioned group opinion once a discussion of relevant topics had begun. Talland (1955) also examined the verbal interactions that take place in therapeutic groups. He used the method of interaction process analysis devised by Bales (1950) and showed that verbal exchanges proceed on different lines from those in problem-solving groups. Of great interest are Talland's investigations (1953, 1954*a*, 1957; see also Talland & Clark, 1954) on discussion topics which are considered therapeutically helpful by patients and therapists. There was a large measure of agreement between the views expressed by patients and by therapists in general ($r = +0.71$), but there were some noteworthy differences. Patients did not think as

highly of the therapeutic value of transference topics as did therapists. They had, however, a better opinion of the discussion of symptoms and childhood memories than the therapists. The patients were agreed that the most disturbing discussions were also, by and large, the most helpful ones from a therapeutic point of view. It was the general opinion that it was best to discuss topics which could not be freely ventilated in other social groups in which there was less candour and tolerance.

SOME CONCLUDING REMARKS

In the treatment of mental illness two trends have always co-existed, but have held sway alternately. These trends can be distinguished as impersonal and personal.

Impersonal methods of treatment comprise the main aspects of physical procedures, drug treatment, segregation, custodial care, and the 'institutionalisation' of patients which subordinates them to the demands of the administrative machine.

Personal methods of treatment are more exacting. They require that human interest should take precedence over bureaucratic regulations, however well intentioned. They require initiative and enthusiasm instead of monotonous adherence to routine. They require improvisation and spontaneity instead of contentment with a smoothly running authoritarian administration.

The alternation between impersonal and personal trends of treatment is of very long standing. In the beginning of the nineteenth century the fight against the degrading impersonal regimes and 'treatment' in asylums was inspired by the revolutionary and non-conformist ideologies of the time which sought to liberate the individual and his conscience from the power and influence of secular and ecclesiastical groups. To-day, oddly enough, the encouragement of hospital staff to take a personal interest in patients comes, not from a belief in the value of individuality, but from theories which proclaim the therapeutic supremacy of democratic processes.

In group selection procedures reliance is placed on group processes which are expected to throw up potential leaders. In administrative therapy social incentives (such as approval, encouragement and remuneration) are used to stimulate work performance and participation in group events. In therapeutic communities the anarchic breakdown of organization is utilized to shock patients into a realization of their democratic responsibilities towards the community in which they live. In small-group therapy the cathartic effect of painful group confessions and the relief that comes from shared feelings of guilt are employed.

Whatever the intellectual theories which to-day lead psychiatrists to utilize group processes in the administration of hospitals and the treatment of patients, the one common factor

in all administrative and group treatment is the personal interest shown in the feelings, thoughts and actions of patients. This was as much a therapeutic factor 150 years ago as it is to-day, in spite of differences in theoretical rationalization.

The time will inevitably come when the pendulum swings back again towards authoritarian regimes in treatment. Let us hope that then some of the lessons learned about the patients' need for personal attention and human interest will not be completely forgotten.

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OBJECTIVE OBSERVATIONS OF PERSONALITY DEVELOPMENT IN EARLY INFANCY

By H. R. SCHAFFER*

Genetic explanations of personality tend generally to attach a great deal of importance to the period of infancy, and, mainly through the stimulus of psychoanalysis, much has been written about this stage of development. Direct empirical studies of infants, other than those concerned with the establishment of age norms for certain peripheral functions, are few, and our knowledge of psychological events taking place at this time is for the most part extremely scanty.

The main reason for the lack of data based on direct observation appears to arise from the difficulty of *access* to personality functioning at this age. In the present paper, however, we shall be concerned with a situation which does appear to lend itself to the isolation and study of some of the problems in this area. The observations arose in the course of a project on the effects of maternal deprivation in the first year of life as seen in a sample of hospitalized infants. Here we shall not, however, be concerned with the hospitalization issue as such, but rather with the light which it throws on the personality structure of the infants undergoing this experience. The empirical data of this project are presented in detail elsewhere (Schaffer & Callender, 1958), and only those parts of the study which are relevant to the present theme will therefore be mentioned here.

SUBJECTS AND METHODS

The subjects were 76 infants, admitted to a children's hospital for a variety of medical and surgical reasons. Their age at admission ranged from 3 to 51 weeks, and the length of hospitalization varied from 4 to 49 days, with a mean of 15.4 days. Most of the infants were in

for periods between 1 and 2 weeks, and the median of the distribution is 12 days. Approximately half the children were visited daily, while of the rest only six were not visited at all. Developmental quotients were taken at the end of the hospitalization period with the Cattell Infant Intelligence Scale, and a range of 72 to 141 was obtained, with a mean of 100.7. All cases of possible brain injury and all premature and marasmic infants were excluded from the study.

The observations were focused on three points of the infants' experience—the period immediately following admission to hospital, the period immediately preceding discharge, and the period subsequent to return home. In order to rule out the effects of the illness factor on behaviour, only 'cold' cases were studied during the initial period, i.e. infants not affected by such factors as pain or fever and who were therefore not subjectively sick. There were 25 such cases on whom observations took place for the initial period of the first 3 days. The whole sample of 76 cases was studied for the last 3 days in hospital, by which time it could be assumed that the behavioural effects of their illnesses had disappeared for all babies.

Observation in the hospital took place in the context of a fixed daily observation session, and the data, collected under a standardized procedure, were subsequently analysed according to an Infant Behaviour Schedule. This material is described in detail elsewhere (Schaffer & Callender, 1958).

All the infants were visited at home, always within 7 days after discharge from hospital and also subsequently, until the observer considered that all overt effects of their experience had disappeared. It is the information derived from these home visits which provides us with our point of entry for the present discussion.

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Table 1. *Post-hospitalization syndromes*

	Age in weeks at discharge from hospital														Total
	0-4	5-9	9-12	13-16	17-20	21-24	25-28	29-32	33-36	37-40	41-44	45-48	49-52	53-56	
Global	—	—	2	5	5	4	4	5	1	1	—	1	—	—	28
Overdependent	—	—	—	—	—	—	—	4	4	2	7	3	5	2	27
Miscellaneous	—	—	—	1	1	2	1	2	3	1	1	—	—	—	12
Unchanged	1	1	2	—	2	1	—	1	—	—	—	1	—	—	9
Total	1	1	4	6	8	7	5	12	8	4	8	5	5	2	76

THE POST-HOSPITALIZATION SYNDROMES

The main finding of this study, as shown in Table 1, is the emergence from the information obtained in the home of two distinct syndromes, each closely associated with a particular age range. The cutting point is at approximately 7 months, though there is some overlap. Thus one syndrome, shown by 27 cases, is not found at all below the age of 29 weeks at discharge from hospital, while only 6 of the 28 showing the other syndrome appear above that cutting point. The characteristics of the syndromes are as follows:

The global syndrome. This pattern of behaviour was mainly shown by those under 7 months. When these infants returned home they were, according to the reports of the mothers, 'strange' in their behaviour. The main feature of this 'strangeness' was an *extreme preoccupation with the environment*. For hours on end sometimes the infant would crane his neck, scanning his surroundings without apparently focusing on any particular feature and letting his eyes sweep over all objects without attending to any particular one. A completely blank expression was usually observed on his face, though sometimes a bewildered or frightened look was reported. In the extreme form of this syndrome the infants were quite inactive throughout, apart from the scanning behaviour, and no vocalization was heard though one or two were reported to have cried or whimpered. When confronted with a toy the infant disregarded it.

In the less extreme instances of the syndrome the preoccupation with the environment was again the central feature, but here the infant might not be completely subdued in his general activity, or there might be some vocalization,

or he might momentarily show normal interest in toys before again returning to the unfocusing inspection of his surroundings. Seven of the 28 infants in this group showed such modifications.

Reactions to other people were also changed for the duration of the syndrome, and this applies equally to the familiar mother and to complete strangers. In some cases the infants were quite unheeding of all attempts on the part of the adult to make contact with them, as they appeared to be so absorbed in the scanning of their physical surroundings. In other cases the infants kept the head averted on being stimulated (almost as though deliberately avoiding the adult). In still other instances the infants gazed 'through' the adult with the same blank look that was used for the rest of the environment. Finally some infants were reported to respond to stimulation with a brief sign of interest such as a smile, usually after considerable delay, before again reverting to the scanning of the surroundings.

This pattern of behaviour was sometimes first observed in the waiting room off the ward where the mothers collected and dressed their children prior to leaving hospital, at times as the infants were carried out of the hospital into the street, and sometimes not until entry into their own home. The duration of this behaviour tended not to vary a great deal for the group as a whole—in the majority of cases it continued for the rest of the first day home, but in one or two cases it lasted only 20 or 30 minutes. In some others it continued for as long as 4 days.

Accompanying this pattern a somatic upset occurred in 16 cases. In a few instances this took the form of a feeding upset such as vomiting, but mostly it constituted a sleep distur-

bance, in which the infant would wake in the night crying and would not settle for several hours. In 3 instances, however, the upset took the form of excessive sleeping. In these cases the infant would fall asleep some time after discharge from hospital and continue sleeping for 1 or 2 days, waking only for feeds or even having to be wakened for them.

The somatic symptoms usually outlasted the environmental preoccupation, but if the time of disappearance of the last symptom is taken as the total length of upset on discharge from hospital, a mean of 2.96 days is found for this group. It is noteworthy that neither the total duration of upset nor the duration of the environmental preoccupation was found to be in any way dependent on any of the antecedent variables such as age, amount of visiting by the mother, or even length of hospitalization. Thus an infant hospitalized for 4 weeks would show the same phenomena for the same length of time as an infant hospitalized for 4 days (the minimum hospitalization in this group).

This pattern of behaviour has been named the 'global syndrome' because, first, it appears to be related to the total environment rather than any specific aspect of it (such as the mother, as in the following syndrome). Secondly it tends to involve the total organism, and somatic as well as psychological functions. Finally because it is believed to be indicative of a global, undifferentiated, syncretic stage of development. The first two points are, as we have seen, based on the observed data, whereas the last is a hypothetical point which will be elaborated later on.

The overdependent syndrome. This pattern of behaviour, commonly found after the age of 7 months, presents a very different picture. The central feature here is overdependence on the mother after return from hospital, and it is thus of the same order as that described for older, pre-school children under similar circumstances (Prugh *et al.* 1953). The overdependence was shown in such ways as excessive crying when left alone by the mother, an almost continual clinging and a wish to be nursed by her, and a fear of strangers. Familiar

figures, such as father or siblings, were sometimes regarded with suspicion. A somatic upset was found in 15 of these 27 cases, taking the form in most cases of a sleep disturbance. The mean duration of total upset is 14.69 days for this group, but there is a wide range (1-80 days).

Among the 27 cases in this group are 7 who showed a variation on this theme. These cases developed the global syndrome after discharge, and it was not till this had run its usual course that overdependence manifested itself. There are no obvious reasons for the difference between this subgroup and the other 20 cases. Their age at discharge ranged from 29 to 47 weeks, and length of hospitalization, amount of visiting, and other relevant variables are not correlated with it.

While the above two groups may be taken to represent the main syndromes of post-hospitalization behaviour as seen by us, some infants were found who did not fit into this classification. There are two subsidiary groups.

Miscellaneous. The 12 infants in this group showed only such isolated symptoms as a sleep upset, or fear of strangers, or a decrease in their general activity. There were also 2 cases, both approximately 8 months old, who were overdependent on the mother before hospitalization and where after return home the only behavioural change observed referred to a marked lessening of the overdependence.

Unchanged. Nine infants showed no reaction at all on return home, no change in their behaviour having taken place as compared with the pre-hospitalization picture. Four of these cases are the youngest of the sample, and here our observations on these infants suggested that visual awareness of the environment had not apparently sufficiently developed for them to have shown the global syndrome.

In general, apart from the 4 youngest in the 'Unchanged' group, it is difficult to see why the cases falling into the last two groups do not conform to the pattern set by the first two groups. None of the obvious antecedent variables can be adduced to account for the difference, and the behaviour of these infants in

hospital was in all respects comparable to that of the others. In the remainder of this paper attention is paid, in the main, to the two main syndromes, for it is hoped that through an examination of the main sample we shall eventually arrive at an understanding of the reasons why some cases fail to conform.

COGNITIVE STRUCTURE AND THE DEVELOPMENT OF OBJECT RELATIONS

The main conclusion arising from the material presented above is that the same experience of hospitalization is reacted to very differently, according to the age of the subject. After 7 months the disturbance caused thereby is in the field of object relations, with particular reference to the relationship with the mother, and is thus continuous with that found generally in the pre-school age group. Before 7 months on the other hand, the disturbance is of a very different nature and appears to be related to the total environment rather than to any one aspect of it. The reason for this difference is to be found, we can assume, in certain developmental changes taking place soon after the middle of the first year, and the possible nature of these changes deserves to be examined in relation to the two different syndromes described.

The most useful way of approaching this problem appears to be through a consideration of the type of *cognitive structure* to be found in infancy, i.e. the way in which perceptions are organized and related to each other and to their external sources by the individual. This is a function about which Piaget (1937, 1950) has written most clearly, and his theoretical propositions will therefore be examined in relation to the material presented above. In the early months, according to Piaget, there is present a state of 'adualism', an undifferentiated absolute in which there is no distinction between the self and the environment. Objects at this stage do not exist in their own right but only as functional elements serving the infant's own activities, and are assimilated in terms of the present need of the individual. Moreover, the object is out of the perceptual field the

infant behaves as though it has ceased to exist. He is thus said to experience only a series of fleeting images which may be recognized but which have no continuity, permanence, or substance. There is consequently no conservation of the object, and the world is centred in the child's own activity. There is, furthermore, no appreciation of the own body as one element amongst others, and it is thus not recognized as being part of a world of distinct, stable objects.

It is only in the second half of the first year that Piaget finds a new type of cognitive structure developing. It is only then that objects become detached from action and the first fundamental step taken in attributing to them a separate, independent existence. Though the individual must yet pass through many stages to reach the adult form of cognitive organization, the most important step may be said to take place at this point, for it is now that the body of the subject becomes appreciated as distinct from the environment and relationships to external objects can therefore be established.

This formulation has definite implications for studies on separation from the mother during the first year of life. As Anthony (1956) has put it: 'It is only after the child has made a permanent object that he can lose it, search for it, or form a permanent relationship with it', and from the theoretical formulations of Piaget he went on to deduce that the infant's reaction to separation from the mother must before 7 months lack the quality of separation feelings at a later stage. This is strikingly borne out by the present investigation, even in respect of the actual age stated. If we examine the initial reaction to hospitalization in the group of 'cold' cases, we find a very different pattern before 7 months from that occurring after this age. In the earlier period fretting as a protest to the separation does not occur, and apart from a sharp drop in the amount of vocalization (believed to be related to the general reduction of social stimulation) the infant's responsiveness to the strangers now caring for him is maintained at normal levels. After

7 months, however, one tends to find the classical separation picture, shown in particular by fretting, strong negative responses to the strange hospital staff, and clinging to the mother during her visits. This has been detailed elsewhere (Schaffer & Callender, 1958) and supports the conclusion suggested by the home data that separation from the mother becomes an experience manifestly and immediately affecting object relations only after the middle of the first year. (This does not, of course, prove that deficient mothering before this time cannot affect the development of the child in such a way as to influence later object-relationships.)

There is, however, another point suggested by the data, namely one referring to the speed with which the new development takes place. While these conclusions are admittedly based on cross-sectional material, it does seem remarkable that there is relatively little overlap between the two main syndromes, as though the new function comes into play quite suddenly around 7 months. What is more, there are indications that the intensity of the separation reaction does not increase gradually with age once this milestone has been reached, but that the upset is as great at the very beginning of the new phase of development as it is later on. There are three possible indices of the degree of upset caused by the separation.

(a) *The length of the fretting period.* Basing conclusions on the 'cold' cases, a considerable range (1-22 days) was found. But as many of the infants were still fretting at discharge (including the 37 week old infant who was still fretting after 22 days), no precise statistics can be given. It is notable, however, that the very youngest in this group continued fretting for periods as long as those found at the older end of the age range. Thus of the two youngest, both 30 weeks at admission, one was still fretting when discharged on the 5th day, while the other ceased fretting only after the 6th day. One may compare such figures with those given for older children—for instance Robertson & Bowlby (1952) mention periods ranging from a few hours to 7 or 8 days in the case of children

18-24 months old, and for a group of children aged between 1 and 4 years at the time of separation, Bowlby (1953) describes the fretting period as lasting from 1 or 2 days to 17 days.

(b) *The intensity of fretting at the beginning of hospitalization.* All infants were rated on a five-point rating scale for the amount of crying observed during each daily observation session. Studying the 'cold' cases again, it was found that the average rating for the first 3 days in hospital shows fretting to be as intense in the 7 and 8 months old infants as it is in those aged 11 and 12 months. Thus if the average ratings for the 7 infants aged between 29 and 40 weeks at admission are compared with those for the 8 infants aged between 41 and 52 weeks, mean figures of 3.68 and 3.55 respectively are obtained. It would appear that at the very beginning of the new developmental phase protest at separation is as vigorous as it is for the oldest subjects of our sample.

(c) *Duration of upset after return home.* Examining the group of 27 infants who were overdependent after discharge from hospital, a range of 1-80 days was found for this index. Again the very youngest infants in the group were no less affected than the oldest. Dividing the age range at 41 weeks, the mean duration of upset for those above and those below can be compared. The outcome is, however, influenced by the fact that in the older group three of the longest hospitalizations are found, whereas only one case of similar length is present in the younger group. As length of upset after discharge has in the literature generally been found to be directly related to the length of hospitalization (except in the case of those under 7 months), we felt justified in excluding these 4 cases for the sake of uniformity and comparability of the two groups. In this way a mean of 10.18 days was obtained for the 29-40 week old group, while the mean for the 41-52 week old group was 11.83 days. The difference between these means is not statistically significant.

It is implied by these figures that the intensity of separation upset is considerable at the very beginning of the new phase. Once an infant has

reached the developmental stage where he becomes capable of showing a separation reaction his upset is likely to be as great then as it will be later on. The age range within which we have carried out comparisons is rather narrow, however, and the findings need therefore to be confirmed for a wider age range. Clinical impressions suggest in fact that the 7 or 8 months old infant frets at separation and clings after reunion just as intensely as the 2 or 3 year old child, and if this can be confirmed by systematic studies a *stepwise* development is thus indicated.

It seems justified to assume that the intensity of the separation reaction is an indication of the strength of the child's libidinal attachment to the mother (as a special and unique person, rather than as a 'mothering agent'), and we thus have evidence that the 'common sense' view of a *gradual* development of the child-mother relationship in the course of the first year or so is a mistaken notion. The more likely hypothesis is that *permanent object relations (to specific persons) do not become possible until the cognitive structure of the child has developed to the point where others are seen as separate beings clearly distinguished from the self and from one another, that this occurs somewhere around 7 months, and that when this development does take place the attachment to a specific mother-figure is established relatively speedily and appears at once in its full intensity.*

It is significant that a similar theory of two-stage development emerges from a study of dogs by Scott (1958), who found that despite the close association of the puppy with its mother during the first weeks of life, primary social relations cannot be established until certain maturational changes have taken place. These occur after 3 weeks of age, and it is only during the critical period for the process of socialization which begins at this time that the puppy becomes able to establish permanent social relations. From another point of view Bowlby in a recent paper (1958) on the development of the child's tie to the mother, has also postulated a two-stage process of social development in infancy, according to which

various instinctual components of the child's 'attachment behaviour' to the mother do not become integrated and focused on the one specific individual till the second half of the first year.

ANALYSIS OF THE GLOBAL SYNDROME

If we accept the view that an object relation to a specific person does not become established before approximately 7 months, it is understandable that in the earlier period hospitalization does not produce a manifest and immediate disturbance in the child-mother relationship. The problem arises, however, of why the global syndrome appeared after discharge from hospital, and what light it can shed on the processes underlying it.

In attempting to analyse this syndrome, we may take as our starting point the fact that this pattern of behaviour was elicited when the infants were moved from hospital to home. There are two possible explanations which must first be examined.

The first is that the global syndrome is the usual reaction at this age to all geographical displacement. All the mothers agreed, however, that the global pattern was quite unusual when compared with the infant's customary reaction to new surroundings, that it differed from the latter in its duration and intensity, in the inaccessibility of the infant, and in such other features as the unfocusing character of the environmental inspection and the quietness accompanying it. To check on this point the observations on the 'cold' cases at the time of their admission to hospital were examined, but no trace of any pattern of behaviour resembling the global syndrome could be found.

The second possible explanation is that the global syndrome is specific to the move from hospital to home. One may wonder, for instance, whether it is related to the return to a previously familiar environment, or to renewed handling by the mother. But this explanation too can be ruled out, because it was found possible to elicit the global pattern actually within the hospital by merely moving the infant after a certain period of hospitalization from his

customary place in the ward to another room. This is a development which has certain important implications to which we shall return later. In the present context, however, it indicates that the global syndrome is related to any change of environment following a period of hospitalization.

We may now ask how this effect could be brought about by hospitalization. The hypothesis suggested is that the operative factor in the infant's experience in hospital is one that can best be described as *perceptual monotony*, and that this factor is an essential precondition to the development of the global syndrome.

Close observation of the infants on whom this project is based vividly emphasized the considerable monotony of their perceptual experience. One may mention in this connexion the following four aspects in particular.

(a) The child's illness necessitated his confinement to bed, so that he generally remained in the same environment for the whole period of hospitalization.

(b) The physical nature of the environment was often of a highly constrictive kind. The smallest infants were kept in solid-sided metal cots, others were isolated in cubicles or kept in side-rooms from which they could see or hear little of what went on outside, and only those in the busy general ward had the opportunity for greater perceptual variety.

(c) Even those in the general ward, however, were usually prevented by the limited perceptual-motor equipment of infants in the first 12 months from making use of the greater amount of potential stimulation available. None of these infants could walk, few could stand, many could not sit up, and their sensory range tended in consequence to be limited.

(d) Due to factor (c), infants are generally very much at the mercy of others for the richness of their perceptual experience, yet in terms of human stimulation the experience of these infants was particularly deficient. Apart from the visiting hour (during which few mothers ever picked up their babies), the infants were rarely handled apart from the handling necessary for the relief of physical needs. Moreover,

the feeding situation tended to be of very much shorter duration than in the home.

As a consequence of these four factors, there was considerable restriction in perceptual variation compared with the usual experience of infants cared for at home. While some variation in severity of perceptual monotony occurred from individual to individual, inspection of the data showed that the older infants (who later developed the overdependent syndrome) were also considerably affected, and that the more severe degrees of perceptual restriction occur among them as well as in the younger infants. If perceptual monotony is then to be regarded as an essential precondition for the formation of the global syndrome, it is necessary to account for the fact that it appears to play this part only within a certain age range. To do this, we must return to our discussion of the types of cognitive structure to be found in the first year, and advance the hypothesis, elaborated below, that *it is the interaction of perceptual monotony with the early type of cognitive structure which leads to the occurrence of the global syndrome.*

Given the early type of cognitive structure as described by Piaget (with its state of adualism, in which the self is merged with the environment in one functional whole), one may say that under normal conditions of child care the infant experiences a degree of environmental variation which will keep his perceptual field in a relatively 'fluid' state, for both the external boundaries and the internal characteristics of the field will fluctuate as a result of such stimulation.

Under condition of perceptual monotony, however, the rate of change is drastically reduced, and when an infant in whom the self has not yet emerged as a differentiated unit is confronted for a lengthy period with the same relatively static surroundings, the natural tendency to merge with the environment is thereby emphasized. It is as though under these conditions the perceptual field would tend to become 'set'. The boundaries of the field can be thought of as remaining constant for the relevant period, and the amount of variation in

internal structure will be reduced more or less in proportion to the degree of sensory restriction.

The process of 'setting' which we have postulated becomes apparent when the infant is taken out of the accustomed environment and put into another. The 'set' perceptual field is disrupted and disintegrates, and such disintegration may be experienced as a stress situation—hence the somatic upset found in many of the infants. A new perceptual field must now be formed, and the infant's acute awareness of new sensations is reflected in the intense concentration with which he regards his new surroundings. It is this environmental preoccupation which forms the core of the global syndrome, and the various behavioural features of the syndrome may be said to stem from the disintegration of a perceptual field in which a 'setting' process has taken place and which must now be replaced by a new field.

That sensory deprivation even in adults can result in certain drastic changes of behaviour has been reported by several writers (e.g. Wexler, Mendelson, Leiderman & Solomon, 1958). Judging from the published results, however, it seems unlikely that the experimental conditions used bring about a state comparable to that described for the present sample. In the former case the self is a highly differentiated, independent unit, the dissolution of which could only be affected by the most extreme conditions. In young infants on the other hand, the global pattern can be regarded as an exaggeration of a process occurring all the time, a process of fusion of self and environment, and the complete restructuring of the perceptual field whenever the latter changes.

AN EXPERIMENTAL APPROACH

It was mentioned earlier that it was possible to elicit the global pattern by moving the infant from his accustomed place in the hospital ward to another room. A first consequence of this was that we could now observe the child's reaction rather than being forced to rely on parents' descriptions.

A second consequence was that it became possible to set up an experimental situation in which the global pattern is treated as the experimental variable and an attempt could be made to find the antecedent conditions which elicit it or produce modifications in its appearance. It should now be possible to test the operation of the perceptual monotony factor by ascertaining whether an infant who does receive sufficient stimulation after his admission to hospital will fail to manifest the global syndrome. Moreover (returning to the exceptions in Table 1 who showed miscellaneous or no reactions), it is through the use of this experimental situation that we might arrive at the reasons for the failure of some cases to develop the expected pattern of behaviour.

Although a start has only recently been made on the application of this experimental approach, and no systematic information is therefore yet available, one preliminary finding is perhaps relevant here. In some of the infants older than 7 months, and who showed the older pattern in that they fretted on admission and were overdependent after return home, the global syndrome was nevertheless elicited when this experiment was carried out during their hospitalization. In some cases fluctuations were observed in this respect, in that the pattern appeared on some days and not on others. One possible explanation of this phenomenon which may be tentatively advanced is that we are confronted here with an instance of *regression*. Just as bladder control or feeding habits regress under the stress of hospitalization in older children, so the function with which we are concerned here may well regress in the same situation. With this possibility in mind the original data summarized in Table 1 may be examined. Of the 6 infants aged above 29 weeks at reunion but showing the global syndrome, two fretted when first separated. There is both a statistical and a theoretical association between fretting and the overdependent syndrome, and it may be justified to regard the occurrence of the global syndrome in these 2 cases as well, as a regressive phenomenon. The same may apply to those 7 cases

which showed the global syndrome and then became overdependent, though why both post-hospitalization patterns were shown by some, and only one pattern by others is not clear.

If this tentative finding can be confirmed, the possibility arises that the early type of cognitive structure may provide a reference point for regression and fixation, and may consequently be of psychopathological significance. It would then become important to attempt to isolate the factors responsible for delay or impairment of development from one stage to the next.

CONCLUSION

Two main syndromes, each associated with a particular age range, have emerged from this study of the effects of hospitalization in infancy. In this respect the findings parallel those of Spitz (1945, 1946), and they may be said to suggest the existence of two developmental stages—a *global stage* and a *differentiated stage*. The latter, centering around the differentiation of self and environment, appears to be essentially continuous with the adult form, and only when it has been attained can object relations to specific persons be established. The global stage, on the other hand, is of a very different order, and certain life experiences may thus have quite a distinct meaning according to the developmental phase of the individual. The present study, for instance, suggests that the crucial factor in hospitalization at the differentiated stage is *maternal deprivation*, whereas at the global stage it is *perceptual deprivation*.

Stages of development in personality organization during infancy have been postulated by a number of different writers, and though terminology varies the overlap in meaning is considerable. One may in this connexion mention Melanie Klein's (1952) stages of part objects and of whole objects, Hartmann's (1952) stages of the need satisfying object and of object constancy, and Hoffer's (1952) stages

of the object as part of the *milieu interne* and of the psychological object. Certain similarities exist between all these formulations and the two stages outlined here, but whereas the former have arisen from reconstructive data, the present approach is based on direct empirical observations. The controversy, for instance, about the age of change-over from one phase to the next has probably been due to the indirect nature of the evidence, but while some have postulated this important milestone of development to occur as early as 3 months and others as late as 10 months, the material presented in this paper suggests 7 months as the approximate time of graduation from one stage to the next.

The hospitalization situation has thus provided us with a means of 'diagnosing' the developmental phase of infants, and in the context of this situation it is possible to proceed to investigate the problems and to test the hypotheses formulated in this paper. These concern, among other things, the further characteristics of the two developmental phases, especially of the earlier one; the manner of passing from one stage to the other and the conditions, both organismic and environmental, for doing so; and the possibility of regression taking place once the later stage has been attained. Moreover, the stages may be regarded as a framework within which one can evaluate the varying effects of environmental influences on personality development and against which the growth of social relations may be assessed.

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ANOREXIA NERVOSA IN AN ADOLESCENT GIRL

By FRANCES TUSTIN

As early as 1694 Richard Morton wrote of anorexia nervosa as 'Nervous Consumption' and regarded the 'immediate cause of this distemper' to be in the 'system of the nerves'. The early papers on anorexia nervosa, notably those by Lasègue (1873) and Gull (1873), were concerned with establishing the prime importance of emotional factors in its causation. Both papers give descriptions of the strikingly consistent clinical picture presented by such patients. Later papers accept the psychogenesis of anorexia nervosa, and two important American papers, one by Rahman, Richardson & Ripley (1939) and the other by Waller, Kaufman & Deutsch (1940), demonstrated, by comparative studies of several cases, that such patients show similarity in regard to personality traits, phantasy life and to the importance of specific details in the family situation, as well as in the symptomatic picture they present.

In 1948, Dr Clifford Scott wrote of anorexia nervosa, '...as yet little has been done to try to trace specific connexions between the symptoms and the instinctive life, the imagination and the interpersonal relations'. My aim is to try to trace such connexions by describing the emotional processes which seemed to be associated with the fluctuations in weight and eating difficulties in an adolescent girl who was diagnosed as a severe case of anorexia nervosa and which were observed in the context of a particular therapeutic technique. I shall not be concerned to advocate a particular method of therapy. Following the description of the case, the work of other therapists using different but intensive techniques will be quoted. I hope that this comparison of the material obtained in long-term treatments may throw light on

the unconscious processes that are called into play in shorter forms of treatment or in the cases which clear up spontaneously. It may also help to explain why so many relapses occur after apparent cure. The fluctuations in weight of my patient and the unconscious processes which seemed to be associated with them, may also help us to understand the oscillation between anorexia and bulimia which is sometimes a marked feature of these cases. It will also be apparent that although the onset of the anorexia appeared to be sudden, in actual fact the illness had a long history and was the expression of a deep-seated and long-standing neurotic disturbance. This paper is an attempt to add to our understanding of the factors involved in this disturbance.

In the preparation for writing this paper an analysis was prepared of the full notes made during the first one and a half years of treatment. The material of each session was briefly described in one column and my inferences concerning this material were summarized in the adjoining column. It is from these summaries that the following description has been prepared. A graph (p. 199) was drawn to show gains and losses of weight during this period of treatment. As the description of the treatment progresses, the patient's variations in weight will be related to the unconscious phantasies which I inferred were emerging in her relationship with me. I aim to give a simple descriptive account of the work done with one patient, using the minimum of specialized terms, so that therapists with training different from my own will readily be able to compare their experiences with similar patients.

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Margaret was thirteen when she was first seen on 16 November 1955 in the paediatric

* Revised manuscript received 10 May 1958.

department of a large general hospital, from which, after extensive physical investigations had revealed no organic cause for her non-eating and loss of weight, she was referred to the psychiatric department of the same hospital. Before she started psychotherapy on 9 January 1956, Margaret weighed only 3 stones 13½ lbs. She was cyanosed and the paediatric staff were convinced that if she did not get immediate help she would die.

Her teachers described Margaret as a quiet, well-behaved girl. Her mother confirmed this and said that from an early age she hardly played with her toys, preserving them clean and almost untouched. She also would not eat any food which she suspected had a speck of dirt on it. Her blood circulation was poor and she suffered badly from chilblains in the winter. She had not yet started to menstruate.

In her interview with the psychiatric social worker, Margaret's mother dated the anorexia back to September 1955, the beginning of the new school term when Margaret announced that she wanted to slim because she was afraid that she would be too fat to take her dancing examination in November. She had learned ballet dancing since the age of nine and already passed one examination. She and already stopped eating in spite of bullying and coaxing from both parents. At the same time her class were shown a sex instruction film and Margaret fainted. She said that talking about or seeing blood made her sick and that she did not wish to know about sex or menstruation. In September, too, Margaret became very interested in an aunt's pregnancy and also in that of a neighbour.

Father was in the Navy when Margaret was born during the war. She was breast-fed for four months when her mother's milk stopped as the result of her anxiety and distress when she heard her husband's ship was missing. She did not hear from him for three months and was very depressed during this time, although later she heard that he was alive and in good health. Margaret did not take well

to the bottle and was a sleepy feeder. From then until she was ten years old she was a finicky eater. After that, Margaret still had specific foods she would not eat. One of these was sugar which she refused after mother had explained why she herself had to have sugar to prevent from going into an insulin coma. Mother frequently had to go to hospital to be weighed because of difficulties in stabilizing her on a diet.

Mother had started to suffer from diabetes when Margaret was two years old. When she was three, father was demobilized and mother immediately became pregnant with her second child. During this time Margaret went into hospital with gastro-enteritis. When father first came home there was a good deal of tension between the parents, and mother said that it took them a year to get adjusted to each other.

Until Margaret was six all the family slept in one bedroom. When they obtained a council house Margaret had her own room, but she was sometimes called in to help father to look after mother when she was in an insulin coma.

At the time of referral the family consisted of: Mother and Father; Margaret, 13 years; Robin, 11 years; Dennis, 8 years; Jack, 6½ years. Margaret and the boys were always quarrelling. Father openly said that he preferred the boys and ignored and rejected Margaret. However, when she was in the hospital he visited her and brought her presents. Mother reported that she and Margaret had a very close relationship with each other and that Margaret would tell her how she hated father. She also said that Margaret was like a younger sister with whom she could share everything. However, in the treatment situation it became clear that Margaret had strong hostile feelings towards her mother. Mother had wanted to be a dancer when she was a young girl. She had also wanted to have six children but, because of her diabetes, she was sterilized after the birth of her fourth child.

Mother and father each had a close relationship with their own parents, and each expressed resentment about this attitude in the other. In short, it seemed that the family adjustment was a good deal on the basis of one section uniting against another section—boys versus girls—father versus mother—father's family versus mother's family.

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After the first history-taking interview neither of the parents was seen by the psychiatric social worker. When the parents were anxious they were seen by the psychiatrist who took the medical responsibility for the case. I did not see Margaret prior to the beginning of treatment. When treatment started I saw her three times per week for sessions lasting half an hour which was the maximum amount of time I could give her due to my full programme at the hospital. For the first two and a half months of treatment she was an in-patient in the hospital; after this she returned home and came to see me on the same days as before as an out-patient.

In the early days as we sat together in the often silent room I was caught up in her own despair about herself and felt hopeless about helping her. In addition, my own anxieties were intensified by those of the paediatric and nursing staff who were very upset when she lost weight or stood still, or did not put on weight as fast as they would have liked. The fact that my psychiatrist colleague saw his role as that of absorbing and dealing with these anxieties, contributed an indispensable part to the help I was able to provide this child.

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The analysis of the transference relationship was the main instrument of my therapeutic work with this girl which followed the principles outlined by Melanie Klein. This means that both the negative and positive aspects of the transference relationship were taken up and that reassurances were not given other than the ones that were implicit in the analytic situation. I did not offer food

to my patient or reassure her that it was all right to eat. I did not visit her in the hospital ward, or give her presents, or reassure her against her despair that she was untreatable and unlovable (the integration of this despair into the fabric of her personality being a therapeutic aim). However, I tried always to be on time for her sessions with me. I rarely cancelled or altered her sessions and, as can be imagined, this girl provoked much reflexion in me, both in and out of the analytic situation.

It will be seen that in this technique there is much emphasis on the transference situation which is seen as evoking early pre-verbal experiences, particularly in the preliminary phase of treatment. It is in this phase that it is difficult to make contact with patients suffering from anorexia nervosa due to their extreme reticence and withdrawal. Although many ways exist to establish and maintain contact with such patients at this stage, in the method used here, the analytic situation was not modified and an attempt was made to keep contact with Margaret by understanding in detail the play of feeling evoked by the analytic situation and to talk about this to her in simple and direct terms.

It will be obvious that in such a situation, where there is a paucity of verbal associations, other details have to be used as evidence for interpretations which are deduced from different, and in some cases more slender evidence than that available when the patient is more verbally communicative. Thus, interpretations in this early period were based upon slight changes of posture, fleeting facial expressions and tiny hand movements, as well as on the patient's isolated associations. One difficulty in dealing with early material in this way is that an attempt has to be made to reconstruct through the sophisticated medium of words, a play of affect first experienced at the pre-verbal stage of development.

The first phase of treatment will be described in some detail in order to demonstrate initial difficulties, and to give a working example of the technique. The succeeding phases

will of necessity be condensed to fit within the compass of the paper.

* * * *

On 9 January Margaret came from the ward to my room for her first session. She was accompanied by a nurse, and in spite of looking as though her skeleton-like body would be broken into pieces by the weight of the hospital blanket draped around her shoulders, she walked steadily and unaided.

In view of her physical weakness it was natural for her to lie on the couch. I sat at the right-hand side of her pillow. Stillness and immobility were the most marked feature as she lay stiffly under her blanket. The bones of her face showed white through the blue tightly-drawn skin. I explained to her that treatment entailed her telling me any thoughts that came into her mind so that together we could try to understand why she was not wanting to eat. She made no reply to this explanation and lay looking straight in front of her. When she made an uneasy gesture I interpreted her anxiety about me as another new and strange person amongst the many new people she had seen since she came into the hospital. She made no response and continued to lie looking straight in front of her.

After a long pause she spoke haltingly and in such a whisper that I had to lean forward to hear what she was saying. As I did so I became aware of the unpleasant smell that came from her body and it was only with an effort that I stayed close enough to hear that her teacher who had been supposed to be coming that morning had not come. I said I thought she was telling me that when she had seen these other people in the hospital she had been hopeful that they would help her, that she had been disappointed that they had not done so and that she had not seen them again. She felt that they had raised her hopes and had then let her down just as the teacher had disappointed her this morning. She was afraid that I would raise her hopes and then let her down in the same way. I then told her I would see her on three mornings a week and that the days would be Monday, Tuesday and Friday.

After this interpretation and explanation she looked a little less pinched and cold and gradually moved her body so that she was curled up under her blanket in such a way that a picture of a baby feeding came immediately to my mind. She stayed like this for the rest of the session. Her next whispered, halting sentence was given when she was sitting up ready to go to the ward. She said she had been weighed that morning and that she had put on $\frac{1}{2}$ lb. I said I thought she was telling me this so that I felt rewarded for the work I had done and so that I would see her again tomorrow.

This first session well shows the mixture of hope and hopelessness which was such a marked feature of this girl's analysis. The increase in weight on hearing that she is to start psychotherapy is similar to the $\frac{1}{2}$ lb. increase which occurred when she first came into the hospital for physical investigations. In the light of her behaviour throughout the treatment, it probably occurred in response to a resurgence of hope that she had found someone who would understand her needs, to give place to despair and loss of weight when her hopes were disappointed and she was left in the grip of her conflicts.

In the beginning of the next session she did not speak but lay looking at me as she had done towards the end of the first session. It seemed very meaningful to her when I said I thought she wanted to be a baby, and that she felt she was feeding from every word that came out of my mouth. After a pause, in response to a slight movement of her body, I said I thought that she wanted to talk to me with her body as she had done to her mother before she could talk. At this she moved nearer to me and spoke in the same almost inaudible, halting voice. She said that she liked watching the dancing on television. I said this seemed to mean that although she did not like to take things in with her mouth she still liked to take things in with her eyes, as she was doing now with me. A flicker of a smile passed across her face and she continued to look at me.

After a long time of looking at me, she

momentarily glanced out of the window and whispered something of which I only caught the word 'exciting'. I responded to this by saying I thought she felt that there was an exciting life going on outside the hospital, something like the dancing on television, and that I took part in this life. For some reason she only dared to take part in it by watching.

After her session on the Friday of that week (13 January) I heard from the ward sister that she had started eating again and was joining the life of the ward. On Monday she weighed $1\frac{1}{2}$ lb. more. It seemed that her positive attitudes to feeding had been reinforced and stimulated by experiences in the transference relationship with me during the week, and had affected her behaviour outside the analytic situation. It would be useful to try to analyse what these experiences had meant to her. We can infer that she felt that she had found someone who seemed willing to spend a good deal of time and trouble on her and this flattered and reassured her against the deep fears which were to come up later that she was unlovable and worthless. She began to hope that all her needs and wishes, however unrealistic, were going to be satisfied. I suspect that my seeing her constituted a reassurance, as though I were saying, 'It is all right, I will make you better'. It soon became obvious that she saw me as a wonderful, omnipotent person who would perform the miracle of making her better and that she felt that nothing short of a miracle would do this. It also became clear later that 'better' did not mean only better in health but 'better' in a moral sense also, and that in both senses she felt out of the reach of human aid. On the other hand, very intense responses had been called into play which had their prototypes in the feeding situation in early infancy and this had become abundantly clear to both of us. Thus the stage was set for the continued evocation of her unconscious phantasies about feeding and the social relationships this entailed, and a reliving of her early problems through the transference relationship to me in the hope of finding new adjustments. In addition, her impulses to live had

been stimulated by her interest in me and my affairs.

I think the situation of being separated from her home and parents, of living the restricted life of the hospital ward, of being in bed and thus dependent upon the nurses for care and attention, of being physically so weak and helpless and of being in a starved condition, predisposed her to re-experience very vividly the babyhood feelings that we find evoked in all our patients by the special circumstances of the analytic situation.

On Monday and Tuesday (16 and 17 January) she gave more associations and spoke more audibly. I gathered from these associations that she wanted to come so close to me that we were like inseparable, identical sisters and so did everything together. I pointed out to her that by this means she hoped that she might participate in my exciting outside life. In the Tuesday session her associations showed that she was troubled because the breaks between the sessions threatened the closeness she desired and aroused feelings of frustration and anger. However, she preserved our close relationship by directing her angry feelings towards her father and her brothers. After interpretation, she saw how she behaved similarly in the family situation.

On Friday I pointed out to her how she was still trying to preserve the illusion of our identical 'togetherness' by urging me to visit her in the hospital ward to stay with her there. On Monday 23 January she weighed $1\frac{1}{2}$ lb. more. It will be seen that these gains of weight had been possible on the basis of an illusion that she was identical with me as a life-giving, powerful person.

On Monday and Tuesday, 23 and 24 January, she ceased to look at me and was withdrawn and reticent. On Friday 27 January it became clear that this was because she could no longer avoid the fact of our separateness. She said directly that in the times when I was not with her she felt that I had an exciting life in which she could not share. It was clear that this aroused her anger. She was afraid to be angry with me and wanted to produce some-

thing to please me, but because of her feelings of admiration for me, she compared herself with me so much to her own disadvantage that she felt she could not produce anything good enough. She was constipated and, on this occasion, the analytic situation evoked the feelings she had had in the toilet situation in childhood when she had felt unable to produce anything good enough for the mother she admired so much. Her associations showed that examinations stirred up the same feelings, and we saw some of her reasons for avoiding the dancing examination in November. During this period the ward sister reported that she had withdrawn from the life of the ward and her weight continued to fall.

In the following session she expressed dissatisfaction with her auburn hair and showed openly that she envied me my freedom and my pretty clothes. By comparison she felt restricted and dowdy. She wanted to leave the hospital. In the next session she showed that unconsciously she felt that I was as envious and hostile to her as she felt towards me. Unconsciously, she felt that out of malice and spite I neglected her (did not visit her) and restricted her (kept her in the hospital) to prevent her from growing up and being feminine. This was interpreted to her as a reactivation of similar feelings in infancy directed towards her mother. She was hopeless about this situation where we seemed to mean ill by each other and her weight continued to fall.

On 17 February her aunt came into the maternity part of the same hospital to have her baby and on 18 February Margaret identified with this pregnant aunt to the extent of having severe abdominal pains which, on very thorough investigation by the paediatrician, were found to have no physical cause. This episode is reminiscent of the gastro-enteritis when she was three years old and her mother was pregnant with her second child. It also threw considerable light on her behaviour during the sessions previous to her aunt's confinement. In these sessions she had made occasional references to this aunt. She had also shown great concern about the noisy

rumbling in her stomach. It was now clear to me that one of her unconscious phantasies had been that I was the pregnant mother in whose exciting experiences she could share if we were identical and inseparable.

On Monday she did not look at me and was very reticent. The isolated and halting associations she gave showed that she felt she had taken in my words (eaten my food) and so had been able to make progress, to put on weight, to get fatter. In her unconscious phantasy this was equated with making a baby at my expense. We saw now one of the root causes of her non-eating. Ever since she had begun to eat again, she had been feeling that in secret she was growing the baby she produced in phantasy on 18 February. There was also material to show that she felt I preferred boys to girls. As a girl she feared I would punish her as a potential rival or as a disappointing sexual partner. We saw now that one of her many reasons for avoiding the dancing examination was the fear that her femininity would be disclosed. It will be seen later that menstruation was looked upon as a similar examination.

The next session, 21 February, was an important one. Ever since she had become aware of our separateness she had been clamouring that by such things as presents, reassurances and constant visiting (none of which in the end would have satisfied her), I should help to bolster her illusion that I was an inexhaustible source of supplies. She now faced the terrifying fact that she was dependent upon a human being who was heir to all the human frailties such as fatigue, moods, illnesses and even death. The collapse of her pregnancy phantasy, the recognition of the fact of our separateness, and with this, the realization through interpretation of her envious rivalry and consequent hostility, the fact that she was losing weight, the observations she made in the ward which showed that the hospital staff did not always understand how to control other patients' gains and losses of weight by special diets, all worked together to make it impossible for her to maintain the phantasy that she had found a being to care

for her who knew everything, who would gratify her every wish, who was ever-present, who was never ill, never tired, never depressed, who was a source of inexhaustible supplies, whose exciting private life was unspoiled and happy, and who she could always feel was supremely well-disposed towards her. This breakdown in her omnipotence was experienced as exhaustion and tiredness.

In this session Margaret became aware of her feelings of helpless dependence upon me. For the first time she recognized that if she did not eat she would die. She feared that she had exhausted my supplies of good will, patience and understanding (my 'food'), that I would thus withdraw treatment from her and that therefore she would die. As well as being feared because of the envious hostility it aroused, separation from me was feared because it seemed to confirm her deep fears that she had exhausted me. She tried to talk a great deal but she showed clearly her mistrust of her capacity to produce anything that would revive my interest in her.

I saw the feelings of this session as a reactivation of the situation at four months when her mother's milk failed and she was cared for by a depressed and unresponsive mother—a situation which had been reactivated, and the impression deepened by her responses to later difficult situations when, because her mother's attention had been temporarily withdrawn from her, she had feared that she had exhausted her mother's supplies of loving care and attention. Such situations would be the onset of her mother's illness, the return of her father, her mother's difficult pregnancies when she had to be in hospital a great deal, and her experience of her mother's comas.

She had clung tenaciously to the phantasy of my omnipotence because if I were not omnipotent I could be hurt by her attacks and, in her imagination, I became ravaged and despoiled. If she dared to entertain the thought of my fallibility, I could die and she could die. United and identical with me as this mother, she felt possessed by an ill, almost dead mother from whose grip she was unable to free her-

self. Thus she felt hopeless, exhausted and almost dead.

Material in later sessions confirmed that she felt she exhausted me by her greedy demands for my care and attention, and that she had the unconscious phantasy of herself as the baby inside me who exhausted me by greedy feeding and insatiable demands for love, care and attention, and who spoiled my grown-up life by her envious greedy attacks upon it. The anxiety about these attacks was another important factor which contributed to the inhibition of her interest in food and to the restriction of her life. In the session under discussion she was in a state of agitation to produce something which would revive and replenish me so that my care returned to her, but she was desperately afraid that her productions would be ineffectual. It is obvious that her mother's incurable illness must have reinforced the phantasies we saw expressed in the session on 27 January that her productions were worthless and inadequate. We saw now further reasons for her fear of taking the dancing examination in November.

In later sessions we came to see that her dancing was partly an attempt to satisfy her mother's own unfulfilled ambitions, which also contributed to her overwhelming desire to have a baby. In some moods she wanted to satisfy all her mother's disappointed wishes. Her dancing was also an attempt to bring her depressed, dead (comatose) mother to life. It had its roots in the dancing up and down which she had done on her mother's knee and which had pleased and cheered her. Since all movement was equated with life it came to be looked upon as a life-giving activity and was elaborated into her masturbatory movements from which dancing was a derivative. From earliest times her uncertainty had been whether she could (and should) grow, move forward, dance, be lively, be 'playful' (her own association to feeling better), or whether she should stand still, regress, depress her playfulness, stop growing, die. To decide to grow and to live, among other things, meant draining her mother's resources and facing the problems

that this created for her. It also meant satisfying her feminine impulses and, besides bringing her into rivalry with her mother, disappointed some of her mother's wishes. In her treatment she again faced these problems in the transference relationship with me. Her agitation in this session was over how she could get the care and attention she needed because of her helpless dependence without draining me of all my supplies and without arousing my frightening envy and hostility.

In the next session she dealt with this problem by becoming the submissive docile child. Her angry, greedy, biting impulses were seen in another girl in the ward who was 'naughty and bit Sister'. Her rebellious feelings were inhibited and I was seen as the mother who quite rightly restrained this child who was liable to be so destructive. She was shocked by her biting impulses and submitted to restraint out of terror of the possible consequences of her greedy and destructive wishes. In this fear of her biting impulses we have another potent cause for the restriction of her eating. This adjustment, although unsatisfactory, allowed her to feed from me so long as she did not bite. Thus she kept me unused and untouched as her mother had described she had preserved her toys in childhood. Also during this time she told me that she rolled the food around in her mouth until it was soft enough to be swallowed, i.e. she did not bite.

In the few weeks that remained before the Easter holidays she again put on weight, and it was decided that she was strong enough to be discharged from the hospital ward to continue treatment with me as an out-patient. It will be seen from the graph that during the holiday her steady progress was continued. This happened during every long break in treatment. It was noticeable that before a holiday she always made great efforts to adjust her relationship with me so that she left in a mood in which the unconscious phantasies of our having a good reciprocal feeding relationship were uppermost. It became clear that she could tolerate separation if she were sustained by the phantasy that we each drew upon an in-

exhaustible source of supply which meant we could feed, restore and replenish each other endlessly.

Her idea of a 'good' relationship was one in which we gratified each other endlessly. When I frustrated her wishes, however unrealistic they might be, I became the epitome of all that was malicious, spiteful and restrictive; for example, when I was guided by the paediatric staff as to when she could leave the hospital. She responded by revengeful, punishing behaviour and then she felt we were locked in a destructive relationship that boded ill for both of us and from which she feared there was no escape. She mistrusted our intentions towards each other and unconsciously feared that I would poison her as she felt she poisoned me. In these periods she lost weight presumably because she feared to eat.

If, on the other hand, my behaviour made her feel that I was fitting in with her wishes she responded by feeding me as she felt I fed her and by putting on weight, which when she did so, was felt to be something that could be offered to me as a gift. For example, on 18 June I suggested that she should come twice a week and that she should try to do without the Friday session. It will be seen from the graph that there was a great loss in weight following this deprivation. On 2 July I reinstated the Friday session fearing that I was putting too great a strain upon her and because I could not cope with the feelings that were aroused. She immediately responded by drawing a bright, cheerful and connected picture of a table laid for tea. She had started to draw in the second phase of her analysis. Her first pictures had been of stiff, isolated but whole objects, with no apparent theme, similar to the verbal associations in the early sessions. After she was weighed on the following Monday she told me with great pride that she had put on $1\frac{1}{2}$ lb.

At this time, when the relationship was 'good', she felt that, quite concretely, we gave each other weight. When it was 'bad' she felt that we took it away from each other. She talked a good deal about her mother's periodic

weighings and showed that she felt she was in rivalry with her mother in this area also.

It became increasingly clear that she was seeing me as a person with two distinct facets. Sometimes I represented the lively, 'playful' mother of her phantasies who had a healthy body from whom she wanted to feed. At other times I was the depressed, unresponsive mother with the ill body from whom she feared to feed. Her infantile desire for closeness which was sometimes expressed as wanting to come right inside me, or that I should go right inside her, meant that she felt she had taken into her body my moods and state of health as well as my exciting life. If she felt identical with, or inside me as the creative, healthy, responsive mother whom she could refresh and revive by her efforts and who refreshed and revived her, she grew and moved forward. This meant progress and life. On the other hand, if she felt identical with, or inside me as the incurable, depressed, exhausted mother whom she depleted and attacked and who attacked and depleted her, then she felt unable to grow up. This meant regression and death. These two aspects of herself, and of the mother she felt she embodied, were brought together and expressed in a tic-like gesture of hands and body. She would come forward towards me with a fluttering movement of her hands and then fall back stiffly with her spine rigid against the back of the chair.

Her associations during this second phase of her analysis showed that she felt that the loving relationship in which we were so close that we were inside each other, could all too easily slip over into being the possessive imprisonment associated with the greedy, destructive relationship and from which neither of us could escape. Her associations began to show that in this latter type of relationship she felt that we were one inside the other greedily and enviously biting at the contents of each other's body, particularly the penis. Progress again could only be at each other's expense and the one who made progress made it at the cost of leaving the other depleted and dying. Her ultimate fear was that we should

both be left exhausted and dead. There was vivid material in which she showed that she felt that I was a 'condemned' mother whose inside was 'unfit for babies'. There was also material about her successful dancing companions who were 'in shows', and we began to see that, in her unconscious phantasies, 'shows', examinations and menstruation were equated and feared. Menstruation was felt to be another examination which she feared because of what it would show. It could not be welcomed as a sign of growing up and of her hopes of having children, since it was felt to be evidence of her own and her mother's disappointed hopes that she was a boy. As with the pseudo-pregnancy it brought into the open her secret rivalry with her mother and was also felt to be achieved at the expense of leaving her mother incurably ill, bleeding, exhausted and even dead. It was also feared because it would reveal the 'bleeding mess' in her own inside which in her unconscious phantasy she felt was the result of her secret internal biting of the mother she had condemned to imprisonment there. In addition, it confirmed her worst fears that her own inside was bleeding and a place that was 'unfit for babies' as the result of being identical with a mother whom she had attacked in such envious and hostile rivalry. Evidence from the analysis was now coming together sufficiently for us both to be able to begin to understand why the anxieties associated with the events described by the mother in the history-taking interview experienced at a critical time in her development had been so overwhelming as to precipitate her into an illness which represented a life-and-death struggle about growing up.

Up to this stage in her analysis her unconscious conflicts about growing up had been expressed mainly through her body in such things as gains and losses of weight, and the tic-like gesture I have described. Her unconscious feelings of rejection were expressed in colds which occurred on the days when she did not see me. Before the end of a session, her eyes would water but she showed none of the other physical signs of tears and gave no

sign of having the emotions associated with weeping. She would say in explanation that her eyes always watered when she felt cold and this could even happen when the room was warm and she had no cold. Throughout all this time she had related to me on a fantastic and unrealistic basis and her progress was insecurely founded. It was not until September and October that a crisis in her relationship with me modified her repeated patterns and she began to relate to me with more realism in her perception of both of us.

I propose now to give the material of the September and October sessions in some detail. The crisis began with a session on Friday, 21 September when she came looking pink and vivacious. She said she had started a ballroom dancing class which met on Friday evenings and she wondered whether her Friday appointment could be changed and whether she could leave early that day. I let her go early, and unfortunately, accompanied by a colleague, I boarded the same bus as my patient, who felt very rejected by observing my relationship with someone else. The Friday appointment had become increasingly inconvenient for me, and, in the light of her request, I made other arrangements for the Friday and freed other times for her.

On the following Monday, 24 September, she came looking pale and depressed. (Her weight was 71 lb., which was the same as the previous week.) She said that she had a cold and had ear-ache in her left ear which made her deaf. As we sat down in our usual places (we now sat side by side) it turned out that the deaf ear was the one that was nearest to me. It seemed very meaningful to her when I interpreted that she was shutting her ear as she had shut her mouth in babyhood because she wanted to reject my food because she felt I had rejected her by letting her go early on the Friday and by having a relationship with someone else. To hear this interpretation she turned her head so that her other ear was towards me, but after this she said her ear-ache was better and she listened to me with her left as well as her right ear.

We then discussed the Friday session and it appeared that the dancing class started later than she thought and that she could now use the Friday time. I explained that I had now filled in the Friday time and offered her several others. She gave reasons why she could not manage to use any of them and decided to try to do without the Friday session. It was typical of her behaviour that if I could not give her the exact thing she wanted, she could not let me have the satisfaction of giving her a satisfactory alternative. In this case she left me feeling anxious and guilty about a situation which she herself had created.

The next day, Tuesday, 25 September, she said her cold was better but she looked thin and pale. She talked fairly freely and for the first time her associations were concerned with the work she wanted to do when she left school. She also talked about a former friend who had come to live near her and with whom she wanted to renew the friendship. (Up to now she had no close friends.) Towards the end of the session she gave me a long association, talking with a new fluency and describing for the first time an incident in her childhood. The gist of this story was that she had nourished the illusion that she could help herself to things in shops, and she suddenly found out that she had to pay for them. Her eyes glinted with amused resentment when she thought of how mistaken she had been. In this session she also showed that she was grateful for the help she had had from treatment and that her mood was one in which she was feeling disillusioned with me in the same way as she had done about the shop. She had had the phantasy that we were completely at each other's disposal. By seeing us both as equal in this way she had avoided all feelings of envious comparison and of dependency. She was now feeling that I possessed certain things that she would like to have and recognizing that she had to be willing to spend something (to suffer some frustration) to get them.

On the following Monday, 1 October, she had lost 1 lb. in weight and on 8 October she had a cold and saw me as a punishing, retaliation-

tory mother who had taken away the Friday session to punish her for going to the dancing class and for having a life of her own. It became clear that one of her reasons for asking to leave early for the dancing class and for mistaking the time it began, was that she still had a grievance about the earlier occasion when I had suggested that she should try to do without the Friday session. She hated and feared the feeling of being dependent on me and wanted to triumph over me and to show that she was now developing an interesting life of her own. These feelings of grievance, of envious rivalry and of triumph, conflicted with her feelings of gratitude and the desire that I should have pleasure and freedom. On 9 October the tic-like movement had returned and her associations showed that she was again in conflict about whether to make progress or to regress. (It will be seen from the graph that her weight stood still.)

On 15 October her weight was unchanged. Her associations were concerned with the former friend she had mentioned on 25 September. She wanted to get in touch with her but feared a snub. She then showed that she had similar feelings towards me. She wanted 'to take the plunge' and to risk the pain she feared if she allowed herself to experience loving feelings towards me. She also wanted to sleep with me as on one occasion she had slept with her girl friend.

On the following day, 16 October, her mind was full of her brother who had come into hospital for a tonsillectomy. I was seen as associated with the surgical activities of the hospital and she had strong unconscious fears that I would cut into her and take away parts of her body, in retaliation for the predatory attacks in which she felt that, among other things, she had cut out the baby brother from my pregnant body. If I talked about this she winced as though I were actually doing it to her. Her eyes watered copiously and she had pains, first in her throat and then in her back, which went away after I had interpreted her fear that I would attack her. This was a dramatic session and we began to see that to

some extent she was re-experiencing fears which had originated and been elaborated from situations when she had been bathed, dressed and toileted by her mother and had felt very afraid that parts of her body would be taken away. In one way, my acceptance of the situation where she had no Friday session had seemed like an attack upon her—an attack which was aimed at all her sources of pleasure. At the end of the session she told me how her aunt's baby 'cried and cried' when her aunt went out of the room. She nodded in intense agreement when I said that she was feeling that she 'cried and cried' because it was the end of the session. Her eyes were watering as she did so, but she had no other signs of weeping. During this week she lost 1 lb. in weight.

On the following Monday, 23 October, her associations were about leaving school and what work she would do. She had been to tea with the friend she had mentioned previously. On another occasion she had worn lipstick. All her associations were in this vein of growing up, having a life of her own and daring to be feminine.

It will be seen on the graph that during this period, 21 September to 24 October, her variations in weight reflected the swings in mood she felt towards me. On Tuesday 24 October she again talked about her brother's tonsillectomy and again I interpreted the unconscious fears that I would cut into her and take away parts of her body. The deprivation of the Friday session had aroused feelings associated with the frustrations she had experienced in babyhood and her unconscious phantasies were concerned with biting her way greedily into my body to take out the desirable supplies I had there. Her unconscious feelings were that I had done the same to her. On previous occasions she had dealt with the recognition of the 'bad' relationship she felt to be going on between us, either by projecting her aggressive feelings or by using all the facts she could muster to bolster up the phantasy that we could each create inexhaustible supplies that were readily available and with which we could replenish each other endlessly. But on this

occasion, as I interpreted to her, she put her head on her arms on the table in front of her and sobbed bitterly. She was obviously experiencing painful feelings of sadness. As her sobbing subsided, she said as she wiped her eyes: 'It's that I'm afraid I shall always be like it'.

It will be seen that when she came on the following Monday her weight had increased by 2½ lb. From then onwards her increases were steady until normal weight was achieved. After this there have been no undue fluctuations in her weight. The tic disappeared and she had no more colds during the rest of the year, although the colder weather was now upon us. She did not have aches and pains during her sessions. Her eye watering ceased and in situations where this had formerly occurred she cried with all the other physical signs associated with weeping and as though it were associated with emotion. Her associations in the sessions immediately following 24 October showed a new attitude of mind of being able and willing to tolerate psychic and physical pain for a desired outcome, i.e. she now operated more according to the 'reality principle' as described by Freud in his paper 'Formulations regarding two principles of Mental Functioning' (1911). This institution of the reality principle was achieved at the critical point in development which Melanie Klein (1934) has described as the 'depressive position'. This describes the developmental stage at which the developing human being experiences more intensely the reality of his own feelings and thus experiences guilt, sadness and concern because destructive impulses are felt towards loved people on whom he is dependent.

This integration did not mean that the analysis entered calmer waters (Bion, 1953). Margaret began to show more openly and to experience more directly the mood swings and conflicting feelings she had previously expressed through somatic rather than psychic channels. She began to show the neurotic behaviour of which the anorexia had been a physical expression and against which it had

been a defence. Material came up to show her terror of and guilt about her sexuality. There was a good deal of material about her blood circulation and its association in her mind with menstruation. There was also material concerning her sexual wishes towards her father and her desires to have a penis. She suffered from sleeplessness and found it a great effort to come to analysis. However, she always managed to come and her material was easier to understand in that she talked more coherently and fluently.

Her life outside became fuller and more interesting. Her school achievements were of a higher standard than they had been before her illness, and she scored 90 % in a type-writing examination, which meant that she was top in her form in this subject. On leaving school she secured a post in an office where she seemed to have friendly relationships with other girls, although these seemed to some extent to be dependent on having one girl who was her *bête noir*. After starting work she continued treatment with me on a once-a-week basis. This involved a long bus journey to my house instead of to the hospital but she managed to come in spite of a good many obstacles, although she still had phases when she felt very hostile towards me. She weathered the crisis of a pregnancy and subsequent long illness on my part and, at the time of writing, is coming to see me once a week. The current problems are centred around her menstruation which has made a tentative beginning but is not yet regularly established.

* * * *

This case demonstrates very well those features of personality, emotional preoccupation and family situation which the earlier writers showed to be so typical for anorexia nervosa. There is the family where there is an exaggerated concern about food (in this case because of the mother's diet) there is the feeling of not being wanted by one of the parents and there is the intense rivalry with her brothers. Prior to treatment, Margaret presented all the personality traits which had been described as typical for patients who develop this illness.

She showed the stubbornness, the scrupulous cleanliness, the reticence, the withdrawal, the difficulty in making friends and the intense retreat from sexuality. In addition, she illustrates the predisposition of adolescent females to develop this disorder, although cases of boys have been reported. Her symptoms of non-eating and loss of weight were associated with the other typical symptoms—inhibition of menstruation, gastro-intestinal disturbances, poor circulation and vomiting. In the treatment situation she showed the 'obsessive, compulsive and depressive' features described by other writers. She was also preoccupied with the pregnancy phantasies which she acted out through her body, which the early psychiatric investigators found to be so characteristic. She also exhibits other phantasies and details of family pattern which were not in the earlier descriptions. Her material will now be compared with that of other cases who were treated by long-term, intensive therapeutic methods to see if they exhibit similar features.

In one case treated along the lines of a universal collective unconscious, the therapist (von Weiszaecker, 1937) came to the conclusion that 'one of the various ways in which patients might deal with conflicts about their relationship to the mother and father, to imprisonment and freedom, to pregnancy and sterility, and to life and death was by becoming anorexic'.

In 1943 Lorand reported a case treated by classical psychoanalysis. His patient was in her early twenties. He says:

In this patient this symptom complex seemed to indicate a serious disturbance. It was an expression of many conflicts besides those which referred to the sexual sphere—which some investigators maintain is the outstanding or main symptom. There was deeper struggle going on within the patient—a struggle involving not only fight and defence against sexual drives, but drives which were more diffuse and pertained to disturbances in the whole personality structure. These referred mainly to the very early period of the patient's attachment to her mother and successful therapy resulted from the solution and working

through of this early attachment in detailed analytic therapy.

He also says

In her early childhood she remembers being like a little beast who wanted to eat up and tear up everything and everybody. The desires which primarily concerned the mother's breast, food and getting love from the mother became in later stages identified with desires to have everything the mother possessed, including father's love. This in turn was associated with early ideas of oral impregnation.

In another place he says: 'At times she felt herself like a cannibal having the desire to strangle, to yank out the penis.' He also says: 'From the therapeutic aspect, depression is one of the most difficult symptoms to handle, and is responsible for the strong wish of these patients to die.'

In a paper published in 1945, Emmy Sylvester describes in fascinating detail her play analysis with a four-year-old child who showed acute symptoms of psychogenic anorexia. She writes: 'During the first period of her treatment her intense wish for gratification by way of undivided possession of a mother figure was clearly expressed.' Later, after a detailed description of the child's play with a doll, she says: 'However, the significant point is that the rage against the pregnant mother evokes these phantasies and these are benevolently modified only when the analyst lovingly repairs the mother doll.' Later she shows: '... her envious, aggressive, incorporative attitudes towards the males of the family ... in order to be as close to mother as they are and to have their advantages'. During this time she modelled a mother doll 'with both titties and stickies'. She then became like a baby 'a level at which gratification was not yet endangered by her hostile competitiveness'. 'The following period is characterized by new phenomena of ego-growth when her depressive reactions to the first separation from the analyst are worked through.'

Emmy Sylvester summarizes thus:

Her neurosis showed the characteristics of a depression. The conflict arose on an oral level

from her inability to integrate incompatible libidinal and destructive tendencies, both of which were expressed through incorporation. The decisive process in the treatment was the strengthening of the ego to an extent that enabled her to separate her loved and hated objects, and to deal with them differentially on a realistic basis rather than on the former basis of a delusional fused incorporation.

Dr George Gero writing in 1953 describes his work with a female adult patient who had a history of long standing eating difficulties and phobic symptoms. The anorexic symptoms went back to her fifth year when her mother gave birth to a much desired boy child. He describes the girl's struggle against accepting her feminine role, her dissatisfaction with her body and her comparison of her body with her mother's. He says: 'The body image of the mother was split into two contradictory aspects; from one point of view it represented the powerful body with the magic quality of growing things in her body, while from the other it appeared as a destroyed bloody mess.' Later, he says: 'Sometimes during the sessions she expressed the desire to take me in, the whole of me—that is she wanted to eat me up.' He later refers to this as the 'libidinal need to merge with the object'. In contrast to this 'she experienced hostile, tearing and biting impulses...directed against the penis'. The patient brought material which made him conclude that 'the drive pattern to tear something from the mother's body and eat it, in this patient at least, was clearly related to her penis envy'. He concludes by saying: 'A complicated set of factors, not all of them necessarily recognizable will decide whether or not an eating disturbance results.'

We are still far from understanding the complicated set of factors which determine the life-and-death struggle we see taking place in anorexic patients, but each reported case confirms or adds to previous data. From the foregoing review, although each therapist presents the material within the concepts of his own theoretical framework, we can see that even when we drive our investigations

deeper, there is still a striking similarity in the material presented by such patients. In particular, we see the major role played by the early longing for a close relationship with the mother and a craving for the contents of her body, including the father's penis and with it the mother's exciting relationship with the father. In my patient this led on to the phantasies of imprisonment mentioned by one other therapist.

This review also brings out the importance of the early conflicts in relation to the mother's body and the failure of such patients to come to terms with the contradictory aspects of their own impulses and of the mother with whom they so closely identified in infancy in the bodily way characteristic of that early period. We see that they have not achieved a stable capacity to tolerate the depression that arises from the fact that potentialities for such contraries as presence and absence, frustration and gratification, love and hate, restriction and freedom, life and death, creation and destruction, health and sickness, hope and despair, fruitfulness and sterility, co-exist in one and the same person. The material would seem to indicate that the way in which they come to terms with this conflict determines the stability of their adjustment.

When Margaret increased her capacity to bear the depression and anxiety associated with bringing these contradictory aspects together, her progress became more stable and she was more able to experience the reality of my feelings as well as her own. The depression that arose at this stage was different from the depression that came from the breakdown of her feelings of omnipotence earlier in the treatment. In the later depression she was far more in touch with the complexity of her own feelings and had marked feelings of guilt and sadness about her capacity for cruel and invidious behaviour, together with a sense of responsibility for her actions.

Edward Bibring (1951) has described in detail the earlier type of depression and has traced it back to the 'infant's or little child's shock-like experience of the feeling of helpless-

ness'. Sylvester is the only writer who gives details of the babyhood history of her patient who, interestingly enough, was reported to have been weaned at four months (as was my patient). In both cases also the father was absent during babyhood. It may be that overwhelming disappointment in the early relationship with the mother followed by a similar intense disappointment with regard to the father leads to the development of anorexic symptoms in individuals who have certain constitutional dispositions. The fact that the baby had an unusually close relationship with the mother which was disturbed by a sudden appearance of the father may also be important.

Dr Sylvester's case may throw light on why this disorder occurs so frequently in adolescence. In her case, after the early weaning, the baby's development was accelerated and she became a model baby. At thirteen months she developed measles which coincided with her mother's return from hospital with the new baby. After this she lost weight, gave up walking, resumed wetting and soiling and had temper tantrums when her mother nursed the baby. Mother dealt with this strictly and, at two, she again became a model child. At three years old, the father returned and the patient, who until then had slept with mother, reacted violently to being turned out by father. When she was four years old and the mother began a further pregnancy, she responded by becoming cranky, disobedient, babyish and then severely anorexic.

It would seem that the adjustment to the complexity of her emergent feelings by obsessional and compulsive mechanisms breaks down with the uprush of feelings caused by her mother's new pregnancy. In adolescence the intensification of the instinctual drives would have a similar result in patients whose innate predisposition and early developmental situation predisposed them to anorexia. The material would seem to suggest that for some reason integrations which result in the development of the capacity to accept the reality principle with regard to psychic factors, particu-

larly the depressive feelings associated with loss, have never been securely established.

Margaret's remark, 'It's that I'm afraid I shall always be like it', indicated that in this later depression her earlier feelings of helpless despair were still present but had now been modified by the firmer establishment of feelings of hope and trust. Her witch-hunting and god-making tendencies were also modified and she showed greater flexibility in her behaviour now that she was freed from the grip of these extreme phantasies.

Since the method of therapy I used is different from certain other methods in that it aims at eliciting and interpreting these bizarre early phantasies, and since in my paper I have concentrated on this particular phase of treatment, it may add to what has been said in other papers if I summarize what these seemed to be.

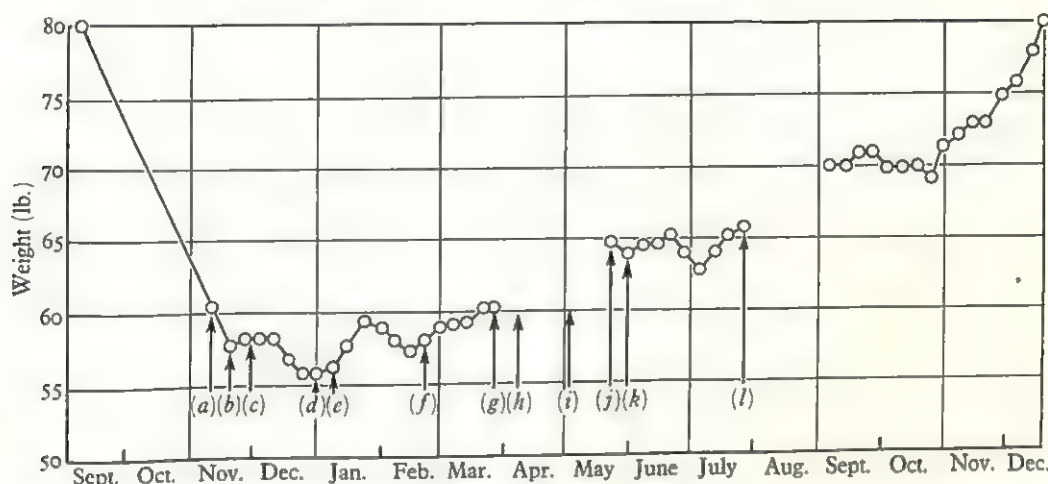
SUMMARY

From the material of my patient I inferred that she put on weight when her unconscious phantasy was of a 'good' relationship going on between us. In this 'good' relationship she felt that we were united and indivisible, omnipotent and indestructible and that we were freed from the restrictions of time and space, and even, I suspected, of our own corporality. She avoided accepting the fact of these restrictions by phantasying that we were one inside the other, feeding each other endlessly, not only with food, but also with much desired grown-up properties (including weight). At this primitive level everything was felt to be passed from one to the other from the inside of our bodies in a quite concrete way. Thus excretions became invested with great significance.

When she was faced with the fact that she was dependent upon a being who had a life apart from her, she lost weight. During this time she felt that supplies were limited and that we frustrated and disappointed each other. In this state of mind her phantasies were that we stole weight from each other, that we cut out the contents of each other's bodies and that

we could only develop by preying one upon the other. She also felt that we imprisoned and poisoned each other. In the state of mind associated with this second group of phantasies she felt that our capacity for destruction was so infinite that there could be no hope of redress. When this despair became intolerable she used whatever she could muster from the environment to evoke the more reassuring phantasies. The working through of these

phantasies in the transference relationship with me, the reality testing and modification which ensued, combined with the consistency of the treatment situation, helped her to develop sufficient psychic integration to recognize that both states of mind were inherent in herself and in the human beings with whom she was associated, and to bear the mental pain of the depression which resulted from this realization.



- (a) 16 November Seen in Pediatrics Department.
 (b) 23 November Admitted to hospital.
 (c) 29 November-29 December. Physical investigations.
 (d) 2 January Seen by psychiatrist—told she is to start psychotherapy.
 (e) 9 January Started psychotherapy.
 (f) 20 February Aunt in hospital to have baby.
 (g) 26 March Discharged from hospital ward.
 (h) 26 March-9 April. Easter break.
 (i) 9 April Returned to school.

- (j) 21 May Weekly weighing recommenced.
 (k) 28 May Whitsuntide break.
 (l) 30 July Summer break commences.

Personal data

Date of birth: 2 March 1942
 Age on referral: 13 years 10 months.
 Height: 5 ft. 2 in.
 Estimated normal weight (as reported by mother): 80 lb.

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ASSESSMENT OF THE FEEBLEMINDED

By LYDIA MUNDY* AND A. E. MAXWELL†

The purpose of this paper is to clarify the relationship between the estimates obtained from the three mental tests—the Wechsler–Bellevue, the Stanford–Binet and Raven’s Progressive Matrices—in most frequent use *clinically* for the assessment of the intellectual ability of adolescents and adults. That such clarification should be necessary at this late date in intelligence testing may seem surprising, but experience has shown that I.Q.’s obtained by the use of these tests are still frequently thought, by some medical practitioners, to be interchangeable and often—to the serious disadvantage of feeble-minded patients—are used in this unenlightened way.

As this investigation is a retrospective one, using data collected by one of the writers over a five-year period, some of the patients had not been tested on all three tests. As a consequence, and to enable all the data available to be reported, it was decided to consider the investigation as threefold. The data presented comprise comparisons: (a) of the Wechsler–Bellevue Intelligence Scale Form I (1944) with the Stanford–Binet Terman–Merrill Revision, Form L (1937); (b) of the Wechsler with Raven’s Progressive Matrices (1938); and (c) the results of a group of individuals are registered to whom all three tests were given. The number of subjects concerned seems large enough and their range of age and of I.Q. level sufficiently wide to indicate suitable norms for the dull-normal and feeble-minded populations.

Acceptance of the recent recommendations of the Royal Commission (on the Law relating to Mental Illness and Mental Deficiency) will

avoid certification to a great extent, but the decision of whether a person is to be regarded as subnormal will still have to be made. Therefore the technical knowledge possessed by the disciplines concerned will remain of some importance.

THE PROBLEM

The problems in the assessment of the feeble-minded are manifold. It has already been shown by Clarke & Clarke (1954), and in a similar study by Mundy (1957), that this group comprises some of the most neglected and deprived individuals of society: frequent changes of home, or an utter lack of a family background may cause maladjustment with emotional immaturity and educational backwardness. But the clinical psychologist will take these aspects into consideration when trying to assess such a person.

However, observations over several years show that not all medical practitioners use standardized mental tests to assist diagnosis. Those who do not use such a test seem to be guided in their decision about their patient’s mental ability by his/her educational achievements, or rather by the lack of them; they appear to equate educational backwardness with mental deficiency. When reading and writing abilities are lacking, or when there is only a rudimentary achievement in the three R’s, the matter seems to be a simple one to these assessors, as normal intelligence has not been demonstrated. Frequently, too, completely unstandardized general knowledge questions are asked, concerned with, for example, the capitals of countries and their distances apart (e.g. London to Paris, or to Dublin, or to New York). Comments, too, like ‘she knows we have a Parliament situated in London but not what Parliament is’, are made as a proof of

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mental deficit, while a favourite arithmetic question seems to be 'how many halfpennies are there in a pound note?' An examiner who asks such questions as these seems to have the standards of his own family and acquaintances in mind; but it is to be expected that people with dull-normal to low average intelligence with a poor social background and schooling, and when a little flustered, could hardly do well on such questions.

On the other hand those who use a standardized test seem to regard the Stanford-Binet (Terman-Merrill Revision) as the 'ultimate standard' in mental testing. To these, one must answer that an I.Q. obtained from any one single test is neither an infallible nor an absolute measure of 'intelligence', and so does not necessarily agree with an I.Q. obtained from another test. Quite apart from the margin of error associated with any mental measurement, the estimate of intelligence obtained is known to be dependent on many variables, some of these being related to the construction of the test itself. For instance, it is known that the Stanford-Binet is verbally biased and influenced by schooling and social background. Therefore a low result on this test by a testee with adverse social circumstances and who is backward in school subjects, may be an outcome of these very aspects, and not the expression of low mental endowment.

It is equally well known that emotional disturbance reduces the quality of mental function. When, therefore, the subject's behaviour in the test situation is described as tense and anxious ('... mostly biting her sleeve or chewing the pencil...') and such disturbed behaviour is not taken into account in the resulting low estimate, the validity of such an assessment would raise doubts in the mind of any experienced psychologist. Thus, the interpretation of a low result on a mental test necessitates adequate technical knowledge and clinical acumen.

However, concerning the technical aspect, it will be shown here that even standardized tests, administered by a professional psychologist may give different results with the same

testees. In particular, the statistical data presented show a significant difference between the Wechsler-Bellevue and the Stanford-Binet assessments. The latter test appears to give a considerable underestimate of intelligence with the kind of patient considered here, and which is related both to age and to I.Q. level. The Wechsler-Bellevue results seem supported by those of the Progressive Matrices. The reasons for these significant differences have not been investigated here and need special study.

I. WECHSLER-BELLEVUE AND STANFORD-BINET COMPARISON

The sample

The sample consists of 190 subjects aged 16-60, tested on both the Wechsler and the Binet, either on the same day or within only a few days' interval (in no case more than a week). In each case the Binet test was administered after the Wechsler and as a result may benefit somewhat from practice effect. With the exception of a few temporary cases referred for investigation, and six out-patients, they are all patients certified as feeble-minded, and apart from six male patients all are females.

The data comprise all test results obtained by one of the writers (L.M.) on first testing a patient, irrespective of whether this was on admission or after the subject has had several years of institutional life. Later test results after a period of rehabilitation have not been included. The lowest I.Q. limit was taken as 50 on the Wechsler Full-Scale, the range of I.Q. being 50 to 113.

Method and results

The sample was first sorted by age (i.e. 16-20, 21-30, 31-40 years, etc.) to investigate to what extent the clinically observed discrepancies between the two tests might be influenced by age. This seemed the most likely factor, as there is little allowance made in the Binet for deterioration with age, and clinical psychologists regard this test as unsuitable for adults.

Table 1. Means and standard deviations of Stanford-Binet and Wechsler-Bellevue, together with the relevant correlation between the Binet and each of the Wechsler Scales at various age levels

Age...	16-20	21-30	31-40	41-50	51-60	16-60
N...	45	46	43	34	22	190
B.	64.18	67.07	64.95	60.00	57.09	63.48
S.D.	11.77	11.46	11.21	13.67	8.63	11.93
W.F.Sc.	74.53	81.30	81.27	82.29	82.54	80.01
S.D.	13.27	13.42	11.78	13.04	6.27	12.58
r	0.93	0.88	0.96	0.87	0.72	0.81
W.V.	73.97	80.41	77.86	80.62	81.09	78.42
S.D.	11.78	10.48	15.03	11.04	5.98	11.91
r	0.87	0.86	0.49	0.78	0.60	0.64
W.P.	81.15	85.34	86.88	87.85	83.95	84.98
S.D.	15.15	15.13	14.88	13.53	8.86	16.80
r	0.87	0.67	0.87	0.88	0.33	0.63

Note. B. = Binet mean scores. W.F.Sc. = Wechsler Full Scale, mean scores. W.V. = Wechsler Verbal Scale, mean scores. W.P. = Wechsler Performance Scale, mean scores.

Correlations between the Stanford-Binet and the Wechsler-Bellevue Scales at various age levels

Table 1 shows the means and standard deviations of the two tests, as well as the relevant correlations between the Binet and each of the Wechsler Scales, calculated for each age level as well as for the whole sample. Omitting for the moment any comment on the means in Table 1, it will be seen that the standard deviations for each test vary considerably from age-group to age-group (as known from the valuable study by Frazer Roberts & Mellone, 1952), but that for all tests the standard deviations for the oldest group (51-60) are significantly smaller than those for the other groups. This suggests that none of the tests differentiates as well between older patients as they do between those under 50 years of age. Now let us look at the correlations between the Binet and the Wechsler tests. Although there is no clear-cut trend to be seen in the age-groups as a whole, the correlations in the 51-60 group are in each case significantly lower than in each of the other age-groups. This may in part be accounted for by the relatively small standard deviation just noted in this group, but it may

also reflect a certain degree of rigidity in the performance of the older patients. The greatest difference in correlation is found between Binet and Wechsler Performance Scale, when the age-groups of 16-20 and 51-60 are compared, there being a drop from 0.87 to 0.33 between these two groups. Perhaps this is to be expected since loss of efficiency with age is known to be greater on performance tasks than in a verbal medium and the Binet test makes no compensation for this loss.

Significance of the differences in I.Q. between the Stanford-Binet and the Wechsler-Bellevue Scales at various age levels

The Wechsler has been designed to give a probable error of 10, which is equal to a standard deviation of nearly 15; in fact, a standard deviation of 14.69 is quoted. In the Binet, all adults aged 16 and above are assumed to remain at the chronological age of 16, for which age a standard deviation of 16.5 is given. The original Binet results were therefore converted via standard scores to correspond to a standard deviation of 15. This had the effect of raising the Binet scores by 2-4 points of I.Q. (since all were below the standardized mean of 100), the mean

of the whole group becoming three points of I.Q. higher. The significance of the difference found between the corrected scores on the Binet and those on each of the Wechsler Scales was now tested at each age level as well as for the whole sample by the *t*-test for correlated means.

discrepancies occurring between these two tests were also related to the subject's mental ability. The sample was therefore sorted by I.Q. level, according to the Wechsler Full-Scale results. The significance of the differences found between the Wechsler and the (converted) Binet results was then examined by the

Table 2. *Significance of the mean differences between the Stanford-Binet and each of the Wechsler Scales at various age levels*

(t values for correlated means.)						
Age...	16-20	21-30	31-40	41-50	51-60	16-60
N...	45	46	43	34	22	190
W.F.Sc., <i>D</i>	7.49	11.63	13.55	19.03	21.91	13.60
S.D.	5.91	6.01	5.15	6.23	4.85	7.34
<i>t</i>	9.11	13.21	17.56	17.92	21.27	25.87
W.V., <i>D</i>	6.93	10.74	10.14	17.36	20.46	12.01
S.D.	5.10	5.40	5.75	6.82	5.72	7.16
<i>t</i>	9.93	13.80	11.33	14.98	16.83	23.38
W.P., <i>D</i>	14.11	15.67	19.16	24.59	23.32	18.57
S.D.	7.98	7.67	8.63	6.77	8.11	9.14
<i>t</i>	11.85	13.53	14.37	21.17	15.86	28.48

Note. All *t* values are significant beyond the 1 % level. *D* = mean difference, W.F.Sc. = Wechsler Full Scale. W.V. = Wechsler Verbal Scale. W.P. = Wechsler Performance Scale.

Table 2 shows the mean difference, the standard deviations of the differences, and the *t*-values. All differences are found to be highly significant, a result which was obvious from the data. Definite and fairly regular trends can be seen in this table: all mean differences between the Binet and the Wechsler tests increase with age. These increases too, seem to be independent of the spread of the differences, for the standard deviations of the latter remain remarkably constant from one age-group to the next. These results bear out the generally held belief (already mentioned) that the Stanford-Binet, Terman-Merrill Revision (1937) is not suitable for adults; and this is particularly reflected in the increase in mean differences with age.

Discrepancies related to I.Q. level

The next investigation of this sample concerned the hypothesis gained clinically that the

t-test (for correlated means), which is shown in Table 3. It is evident that greater discrepancies may be expected at the higher I.Q. levels. Whereas the mean difference between the two tests is 8 points at the 50-69 I.Q. level it is practically 18 points at the level of average intelligence. Again, the mean difference at each level was found to be significant beyond the 0.01 level of confidence.

Prediction equations

Since the discrepancy between Binet and Wechsler I.Q.'s seemed to vary both with age and with level of I.Q. it was decided to find equations from which the I.Q. of a patient on one test could be estimated when her age and her I.Q. on the other test were known. Equations of the second degree were employed, as these provide a check on possible non-linear regression effects and on possible interaction effects between the independent variables.

Table 3. *Comparison of the mean differences between Stanford-Binet and Wechsler—Full Scale by I.Q. level**

(t values for correlated means.)

I.Q.	N	Mean diff.	S.D.	t	P
50-69	35	8.22	6.98	6.96	<0.01
70-79	51	12.31	6.82	12.95	<0.01
80-89	59	14.47	7.09	15.72	<0.01
90-113	45	17.95	5.58	21.36	<0.01

* I.Q. level according to Wechsler-Bellevue.

A quick and simple way of fitting such higher degree equations to experimental data has been described in detail elsewhere (Maxwell, 1957), so that only an account of how such equations may be used is given here.

The first equation we will consider is that for predicting Wechsler Full Scale I.Q.'s (\hat{y}), given the patient's age (x_1) and her Binet I.Q. (x_2). The equation is

$$\hat{y} = 81.85 + 1.70x_1 + 4.69x_2 - 0.12x_1^2 - 0.10x_2^2 - 0.28x_1x_2. \quad (1)$$

When calculating the regression coefficients in this equation four intervals only for each of the independent variables, age (x_1) and Binet (x_2), were possible owing to the small size of the sample. For age these intervals were: 16-20, 21-30, 31-42, and over 43 years, the corresponding x_1 -scores being -3, -1, 1 and 3; for Binet I.Q.'s the intervals were: 35-57, 58-67, 68-77, and over 78 points of I.Q., the corresponding x_2 scores being -3, -1, 1 and 3, as before. It might be argued that these intervals are very crude: where age is concerned this would be justified, for ages generally are accurately recorded, but for I.Q.'s the situation is different. For example, if the S.D. of our Binet I.Q.'s is taken as 16.5 and the reliability coefficient of the test itself is taken as 0.94 then the standard error of measurement of an individual I.Q. is $16.5\sqrt{(1-0.94)}$, that is 4, and the 5% confidence limits for it are as much as 16 points of I.Q. apart.

To use equation (1) suppose that a patient

aged 35 has a Binet I.Q. of 50, then to estimate her Wechsler I.Q. we substitute 1 for x_1 , and -3 for x_2 in the equation, for 35 years of age places the patient in the third age interval, and an I.Q. of 50 places her in the first interval of I.Q.'s employed.

In equation (1) the coefficients of x_1 and x_2 represent the linear regression coefficients of Wechsler I.Q.'s on age and on Binet I.Q.'s respectively, while the coefficients x_1^2 , and x_2^2 , represent quadratic, or parabolic, regression effects on these variables. The coefficient x_1x_2 takes into account possible *interaction* effects between the independent variables when the prediction is made. These regression coefficients are derived by way of an analysis of covariance which also allows each coefficient to be tested for significance. In equation (1) the coefficients of both x_1 and x_2 , that is the linear regression coefficients of Wechsler I.Q.'s on both age and Binet I.Q.'s are positive and very highly significant. This shows that estimates of Wechsler Full Scale I.Q.'s increase sharply as age and Binet I.Q.'s increase. Neither of the quadratic regression effects is significant but the coefficient of the interaction term x_1x_2 , which, incidentally, is negative, is significant at the 2% level. This implies that for higher age-groups and higher Binet I.Q.'s, and also for lower age-groups and lower Binet I.Q.'s the predicted Wechsler I.Q.'s are somewhat depressed, while for higher age-groups and lower Binet I.Q.'s—and the converse—predicted Wechsler I.Q.'s are somewhat magnified.

The second equation set up is one for predicting Binet I.Q.'s when age (x_1) and Wechsler Performance I.Q.'s are known. It is

$$\hat{y} = 66.35 - 4.11x_1 + 10.80x_2 + 0.38x_1^2 - 1.77x_2^2 - 0.01x_1x_2 \quad (2)$$

In this equation the coefficients of x_1 and x_2 , that is the linear regression coefficients of Binet I.Q.'s on both age and Wechsler Performance I.Q.'s, are very highly significant. The former coefficient is negative, the latter positive, which means that predicted Binet I.Q.'s decrease as age increases but increase as Wechsler Performance I.Q.'s increase.

When estimating the coefficients for equation (2) only three intervals on each of the independent variables were used. For age these were: 16-23, 24-39, and over 40 years, with corresponding x_1 scores of -1, 0, and 1. For Wechsler Performance I.Q.'s the intervals were 50-79, 80-92, and over 93 points of I.Q., with corresponding x_2 scores of -1, 0, and 1, respectively.

The third equation set up was that for predicting Binet I.Q. when age (x_1) and Wechsler Verbal I.Q. (x_2) are given. It is:

$$\hat{y} = 67.12 - 4.20x_1 + 11.31x_2 - 1.79x_1^2 - 0.05x_2^2 - 0.68x_1x_2 \quad (3)$$

In this equation two of the regression coefficients also are significant. That of x_1 , which represents the linear regression of Binet I.Q. on age, is significant beyond the 5 % level, while that of x_2 , which represents the linear regression of Binet I.Q. on Wechsler Performance I.Q. is significant beyond the 0.1 % level. The fact that the regression coefficients of Binet scores on age in equations (2) and (3) are negative does not imply that these two variables are negatively correlated, but it does bear out the comment made earlier that in the Binet test little allowance is made for deterioration with age, so that when predicting Binet I.Q.'s from age and Wechsler Verbal or Performance I.Q.'s a decrement, which increases with age, is required.

II. COMPARISON OF THE WECHSLER-BELLEVUE WITH RAVEN'S PROGRESSIVE MATRICES (1938)

Experience gained over several years indicated that too low an estimate of the mental ability of so called 'feeble-minded' persons was obtained on the Stanford-Binet, rather than too high an estimate on the Wechsler Scale. Such a contention is of course difficult to support without undertaking a complete restandardization of the tests concerned. But one possible approach is to compare the tests in question with others widely used. Consequently, a comparison of the Wechsler Full-Scale results with those of Raven's Progressive Matrices (1938) was undertaken.

The sample

Estimates on the Matrices are usually not regarded as reliable below the 60 I.Q. level, and therefore several members of the initial sample were eliminated and a few subjects (which included out-patients) were added who had been given both the Wechsler and the Matrices but not the Stanford-Binet. Thus, the sample of this comparison comprised 180 subjects, age range 16-60, I.Q. range 60-113.

Method and results

Raven does not give a standard deviation for the Matrices but only raw scores and approximate percentiles. At the department of clinical psychology at the Institute of Psychiatry, these percentiles have been transformed into equivalent I.Q. levels by a somewhat complex process of interpolation with the assumption of a mean of 100 and a standard deviation of 16. These I.Q. equivalents are in common use by some psychologists trained in the department.

It was found that the mean differences in I.Q. between the Wechsler and the Matrices were minimal, and this relationship was not altered by the conversion of the Matrices results to correspond to the Wechsler standard deviation of fifteen. Table 4 shows the mean scores and standard deviations of the Wechsler Full-Scale and the Progressive Matrices, together with their relevant correlations, as well

as the mean differences of these scores with the corresponding *t*-values (for correlated means). In spite of its small size, the mean difference between the two tests just reaches the 1 % level of significance for the whole sample, as well as at the ages 16-20 and 51-60. It should, however, be pointed out that in the individual case a difference of 5-6 points of I.Q. is not clinically significant. At the ages 21-50 the mean differences found do not reach significance level.

The meaning of this occurrence can only be surmised from clinical observation, namely that the standardization of the Matrices may be less accurate and the reliability lower below the age of 20 and above the age of 50, especially below 79 I.Q.

interval (the Matrices being usually given first, and the Stanford-Binet last).

This sample consists of 150 subjects, their age range being again 16-60, and the I.Q. range 60-113 (i.e. the lowest I.Q. on the Matrices being 60). As detailed investigations had already been carried out on the previous two samples concerning the relationship between Wechsler and Binet on the one hand and Wechsler and Matrices on the other, only the means and standard deviations of the three tests are given. All scores are directly comparable with the Wechsler, being converted to a standard deviation of fifteen.

It is evident from Table 5, where the means and S.D.'s for the 150 patients who did all three Tests are given, that the discrepancy between

Table 4. *Wechsler full-scale-Progressive Matrices comparison*

(Means, standard deviations and correlations, together with mean differences and *t* values (correlated means) at various age levels)

Age	N	W.F.Sc.	S.D.	Matr.	S.D.	<i>r</i>	<i>D</i>	<i>t</i>	<i>P</i>
16-20	44	81.20	11.06	84.29	10.08	0.72	3.09	3.25	<0.01
21-30	48	83.10	10.70	83.22	9.71	0.89	0.12	0.21	N.S.
31-40	38	83.65	12.32	81.76	9.75	0.72	1.89	1.62	N.S.
41-50	30	84.40	12.42	86.26	11.09	0.86	1.86	1.57	N.S.
51-60	20	82.20	7.26	86.40	6.46	0.63	4.20	2.95	<0.01
16-60	180	82.90	11.08	83.98	9.98	0.79	1.08	2.70	<0.01

Note. W.F.Sc. = Wechsler Full-Scale mean scores. Matr. = Matrices mean scores. *D* = Mean difference.

The correlations between the Progressive Matrices and the Wechsler Verbal and Performance Scales for the whole sample were found to be 0.72 and 0.77 respectively. The mean differences between the Verbal Wechsler and Matrices was 3.25, and between the Performance Scale and the Matrices it was 4.55.

the differences of I.Q. between Wechsler and Matrices on the one hand, and Wechsler and Binet on the other, is a considerable one. These differences are one point and fifteen points of I.Q. respectively (without conversion the latter would increase by three points of I.Q. and the former by 1.5).

DISCUSSION

It is not intended here to enter into a lengthy discussion on the subject of Intelligence and Intelligence Testing, on which there is already a vast body of literature, except to state briefly the authors' theoretical assumptions.

The clearest definitions of intelligence are perhaps those given by Burt (1955*a*, 1957) and

III. COMPARISON OF WECHSLER-BELLEVUE FULL-SCALE, PROGRESSIVE MATRICES AND STANFORD-BINET

As pointed out previously, for a simple and clear-cut comparison, the data of all patients were collected who had been given all three tests either on the same day or with a few days

Table 5. *Wechsler full-scale-Progressive Matrices*(Stanford-Binet mean I.Q.'s and standard deviations at various age levels
(I.Q. range Matrices 60-113).)

Age	N	W.F.Sc.	S.D.	M	S.D.	B	S.D.
16-20	34	80.38	10.38	83.70	9.04	71.14	8.74
21-30	42	83.07	10.94	83.09	9.81	70.69	9.56
31-40	32	84.25	12.52	81.96	10.09	68.50	9.80
41-50	26	84.88	12.75	86.73	10.05	64.53	12.30
51-60	16	80.87	6.49	84.81	12.76	60.31	8.43
16-60	150	82.79	10.90	83.80	10.07	68.15	10.36

Note. W.F.Sc. = Wechsler Full-Scale mean scores. M = Matrices mean scores. B = Stanford Binet mean scores.

by Vernon (1955). Burt uses the term g_i for the innate capacity, and Vernon calls this 'innate quality of the nervous system... of acquiring and recombining perceptive concepts and schemata of all kinds', intelligence A. The actual sampling of this capacity gained by one of the better intelligence tests Burt has called g_i , and Vernon intelligence 'B'. There is also agreement about the environmental influence on the functional level of intelligence, which both writers put at 25-30 %. The point we would like to stress in particular is that made by Vernon: '... that this totality of functioning will differ considerably in its nature and content at different stages of human growth. Our use of the term intelligence had made it appear that we are trying to measure one and the same thing in the 2-year old, the 5-year old, the 14-year old and the 50-year old, which is quite fallacious.'

The Stanford-Binet (Terman-Merrill Revision 1937) has not been truly standardized for adults and should not be used for individuals older than fourteen. Those testers who use this scale for adults seem to think that an adult who has not got the achievements of a normal child of eight or ten, must surely be very backward. But this assumption is quite erroneous. The approach to the adult has to be different not only because adult knowledge greatly differs from that of the child, but also because he will be upset by those 'childish

questions' and will not do his best in the test situation.

Alderdice & Butler (1952) made a comparison between the Stanford-Binet and the Wechsler-Bellevue by factorial analysis in which the Binet was included as a subtest of the Wechsler. It was found, as would be expected, that the Wechsler, and particularly its Performance Scale, measured different aspects of intelligence than did the Binet. But these authors did not seem to realize that it is just this difference which makes the Wechsler Scale the more appropriate measure for adult intelligence.

As already mentioned, some validation of this contention was sought, and Raven's Progressive Matrices were chosen for several reasons. First, because this test is known to be constructed on Spearman's principles, and being comparatively independent of environmental influence, it is usually considered a more accurate measure of 'g' than most other tests. Also, clinical experience indicates that individual administration of this test to this kind of patient makes for high reliability, re-test results at 2-year intervals being practically identical (though a coefficient of correlation has not been calculated). Secondly, the Progressive Matrices are used widely by clinical psychologists for the assessment of the intellectual level of Mental Hospital patients, and are often administered in conjunction with the

Wechsler Scale. It therefore seemed of some interest to examine the relationship between these two scales, even if only at these comparatively low intelligence levels.

The clinical observation which indicated close agreement between these two very different tests seems to be borne out by the results herein reported; and the contention put forward that these tests, Wechsler and Matrices are more appropriate measures of adult intelligence than is the Stanford-Binet seems to be demonstrated.

The practical corollary is that a person registering dull-normal to average intelligence on either the Wechsler Full-Scale or the Progressive Matrices, may appear as subnormal or dull on the Stanford-Binet. The latter test gives significant underestimates of adult intelligence, in particular with this kind of population which suffers from verbal incompetence due to inadequate schooling and poor reading ability, their range of vocabulary and their general knowledge being limited. Whereas on the other hand, the verbal bias of this test may overestimate the g_i of a grammar-school boy.

Tizard *et al.* (1950) used five different tests on 104 adult 'high-grade' male patients, namely the block Design, the Progressive Matrices 1938, the Stanford-Binet Vocabulary, the Porteus Mazes, and Cattell's Non-Verbal Intelligence test Form I.B. Their unusual finding was that in spite of the mean I.Q.'s on the first three of these tests being in close agreement, the correlations of the Binet Vocabulary with all other tests proved to be negative, which the authors found difficult to explain. They thought this phenomenon to be due possibly to the vocabulary test being a better measure of general intelligence than were the performance tests. But this seems an erroneous conclusion, because Koh's Blocks and the Progressive Matrices are known to be good measures of 'g' rather than of the less intellectual aspect of 'practical ability'. In our view the reason for this negative correlation might be caused by the Binet, and its Vocabulary items in particular, being the *worst* measure of 'g' for this population.

In view of the discrepancies demonstrated in this study between the Stanford-Binet on the one hand and the Wechsler and Matrices on the other, and bearing in mind that the differences of I.Q. between the two latter tests, standardized for adults, are extremely small, it appears that the Stanford-Binet is an unsuitable test for estimating the mental level of adults; particularly when they have had an adverse social background and their educational achievements are inadequate.

SUMMARY

In this paper attention is drawn to present methods of assessing the intellectual ability of the feeble-minded, and an effort is made to clarify the relationship between I.Q.'s obtained from: (a) the Wechsler-Bellevue Intelligence Scale; (b) the Stanford-Binet, Terman-Merrill Revision 1937, Form L; and (c) Raven's Progressive Matrices, 1938.

1. The first comparison made is between tests (a) and (b). Means and S.D.'s for I.Q.'s obtained by patients in five different age ranges are given (Table 1), together with the correlations between the tests. The means are compared (Table 2), and it is found that differences between Binet and Wechsler I.Q.'s differ very considerably—the Binet being the lower—and that these differences increase noticeably with age.

2. Next, the patients have been regrouped according to intelligence level (on the basis of the Wechsler results) and comparisons are made between mean scores on tests (a) and (b) (Table 3). The differences were again found to be highly significant, and also to increase as level of intelligence increases.

3. Prediction equations were found for estimating: (i) a patient's Wechsler Full-Scale I.Q., given the age and Binet I.Q.; (ii) a patient's Binet I.Q. given the age and Wechsler Performance I.Q.; (iii) a patient's Binet I.Q. given the age and Wechsler Verbal I.Q.

4. Wechsler Full-Scale results were compared with results on the Progressive Matrices for different age-groups (Table 4). Differences here were not pronounced, though those for

the age-groups 16-20 and 51-60 years were significant. The latter is attributed largely to the lower reliability of the Matrices for the younger and older age groups.

To facilitate an overall comparison between the three tests Table 5 was drawn up. In it the means and s.d.'s for all three tests are given for five different age-groups and also for the groups combined.

5. The discrepancies between I.Q.'s derived from the Wechsler and the Matrices on the one

hand, and from the Wechsler and the Stanford-Binet on the other are attributed to the fact that: (i) the content of the Stanford-Binet is unsuitable for adults, and (ii) the test has never been truly standardized for adult subjects.

In view of these considerations, and the fact that the Wechsler and the Matrices tests tend to show agreement, it is tentatively suggested that the Stanford-Binet grossly underestimates the intellectual ability of the so-called 'feeble-minded', especially in the higher age-groups.

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AN EXPERIMENTAL APPROACH TO THE PSYCHOPATHOLOGY OF CHILDHOOD: AUTISM

By JAMES ANTHONY*

The observation of nearly one hundred patients has led me to accept the existence of psychosis in childhood as a convenient working basis for clinical and experimental investigations into the nature of autistic behaviour. If, within the next decade or two, conclusive studies should demonstrate, let us say, a neuro-endocrine basis for the condition, I for one would be happy to drop the term in favour of something more specific, since it raises more controversies than it silences, in a field by now heavily tainted with adult connotations. Nevertheless, I prefer the term 'psychosis' to the more committed one of 'schizophrenia' which implies some form of relationship to the collection of adult illnesses subsumed under that name. Some authors have used the two terms synonymously and interchangeably. Others, like Bender (1954), have been more apt to stress a difference in usage. According to this latter view, it is apparently possible to be schizophrenic without being psychotic and to be psychotic without being schizophrenic. In the light of our current state of ignorance, such scholastic argument can only be regarded as tendentious. We do not as yet know enough about the beginning and end of the psychotic child to hold such firm opinions about the essential nature of his mental state. We do not know, despite several follow-up studies, what happens to the psychotic child when he grows up. We do not know whether he ever develops the delusions and hallucinations characteristic of the adult schizophrenic, or whether he retains his early retardation and detachment unchanged. The Bender follow-up study would suggest the former, and the Kanner study the

latter, which could imply that two different groups of children were involved. Most workers are inclined to regard the adult and child conditions as distinct and discontinuous entities, even though some studies have reported a high rate of adult schizophrenia among the close relatives of the psychotic child (Bender, 1954), which again suggests a possible dichotomy into inheritable and non-inheritable forms of the illness. One thing is fairly certain: that irrespective of the course of the psychosis the children become to varying degrees damaged adults with, at best, a precarious adjustment to life. Their development may be called 'atypical', but this does not necessarily divorce them completely from the stream of normal psychology. I am here concerned with attacking a conception of childhood psychosis as a bizarre, atavistic condition that transforms the affected person into some kind of psychological monstrosity. My special purpose is the reinstatement of the psychotic within the theoretical framework of our normal practice and the re-establishment of his ties with such clinic neighbours as the defective, the organically ill, the psychopathic and the neurotic patient. In the context of these relationships, that which is unusual and 'atypical' begins to assume a more familiar guise. Symptoms are seen to overlap, and the pathognomonic symptom of 'autism' to be less exclusively restricted to the diagnosis of psychosis.

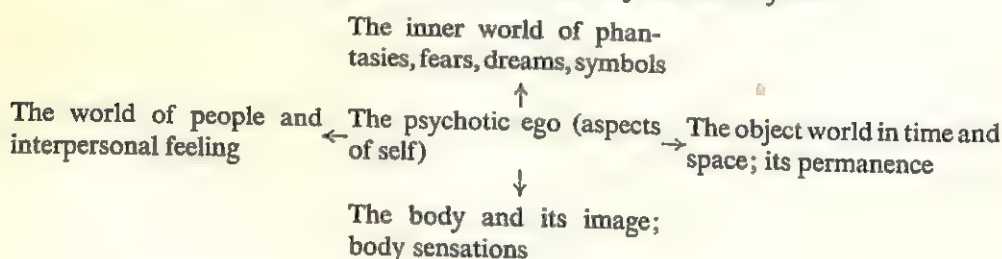
To extend our understanding still further, we must modify our clinical approach to the child. Too often and for too long have we stood outside and regarded him with increasing theoretical bewilderment as his behaviour continued to transgress the laws of orthodox psychopathology. Our only hope at present is to get inside him and to look out at the world

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through his eyes. To be able to do so, we must live with him and share his experiences. This can be done either at home (in America, Jules Henry, the anthropologist, working with Bettelheim, lodges with the family for a period) or, less naturally, in the hospital setting of an in-patient ward. Getting inside the psychotic child is by no means an impossible phenomenological undertaking, if we allow for the growth of empathy that comes only after sustained contact with this type of child. We must

'here-and-now' and space to the subjective needs of the moment; objects will sometimes be permanent and sometimes impermanent, sometimes meaningful and sometimes meaningless, sometimes useful and sometimes useless, sometimes alive and sometimes dead, sometimes the recipients of feeling but more often empty of all love and responsiveness. Quite frequently it is the world of the very young infant set in the twilight of consciousness (Table 1).

Table 1. *The psychotic ego with its defects and defences*



The defects

The failure to relate:

1. The ego to the inner self (asymbolia, absence of dreams, confusion of inner phantasy with outer reality).
2. The ego to itself (lack of self-recognition, self-consciousness, ego boundaries, self-identity; dysidentity).
3. The ego to objects (no object concept, impermanence of objects no conservation or constancy; *a*-dualism).
4. The ego to people (constriction and diminution of feeling, inability to give or reciprocate; *a*-cathexis).

5. The ego to its body (inadequate body image, poor localization of sensation and pain, poor co-ordination; poor representation).

The defences

1. Perseveration, rigidity, 'just-so' behaviour, stereotypy, repetition, circular behaviour, replacement activity.
2. Withdrawal, pseudo-deafness, visual evasion, raised sensory thresholds, going under cover.
3. Transitional relationships to inanimate objects.
4. Projection, introjection, denial.

understand that he is absorbed in demarcating inner and outer experience and in finding a language that will simultaneously serve both abstract and concrete needs, and that anyone with a foot on both sides can in fact be an invaluable go-between. Once inside, his distorted responses will seem much more understandable, based as they are on this system of inner and outer necessity. His world will sometimes be overwhelming in its sensations, sometimes wild and frightening, sometimes flat and uninteresting; time will be reduced to the

The degree to which we can understand will depend on our preparedness to enter into a unilateral and unrewarding relationship. The psychotic child is separated from us by the extent to which we ourselves, the investigators, are alienated from our childhood and from our potential or latent psychoses. The unbounded ego was also once an element in our own development, and having established our boundaries we no longer want to be reminded of that defenceless and vulnerable period when we survived as creatures without shells,

which is indeed what psychotic children remain.

The diagnosis of childhood psychosis has a history. At different stages, different people have reacted to different patients and obtained different perspectives of the condition. The first diagnostic activity lay in differentiating it from mental deficiency. Until this was done, many psychotic children led anonymous lives in institutions and it has taken about thirty years of unremitting clinical work to release some of them from such surroundings. With the first recognition of these cases came a wave of optimism that saw in the differentiation of dementia from amentia the emergence of a reversible state. Time has brought its corrective, and there are clinicians to-day who argue logically that it is wasteful to hospitalize and treat such cases since they respond no better than the defective, and that it would be best to regard them as a subcategory of the amentias. But once a diagnosis has become available, it is difficult to resist making it, especially as it may have a special significance for the parents, who may at times be relieved by it. In one case, a mother said to me: 'I just knew he couldn't be an ordinary defective. His father is a statistician.' She had read enough to know that Kanner had pointed to the eminence of some of the parents in his series, and it was clear that a certain snobbishness had accrued to the diagnosis.

After this first differentiation, a distinguished line of pioneering clinicians built symptom complexes around some nuclear symptom, delineating an apparently distinctive picture to which their name was eventually attached. This cult of names added chaos to an already confused situation, since there did not seem to be a sufficiency of symptoms to share out among the various prospectors without a great deal of overlap. De Sanctis, Heller, Weygandt, Bender, Kanner, Mahler and others all laid claims to part of the psychotic field, and some of them vigorously resisted any territorial encroachment. Kanner (1958), for example, strongly insisted on the specificity of his syndrome, on the uniqueness

of its symptomatology, and on its clear clinical delineation. He protested against the dilution of the concept he introduced and warned that the diagnosis was at present being made 'much too prodigiously'.

The first ten authentic cases of psychosis that a clinician encounters would convince him that he is not dealing with a single disease entity and that his cases fall into different categories. My own concern in diagnosis was to find a reasonable fit for the facts that I had elicited clinically and experimentally, and I looked around for suitable 'cutting points' among the categories to contain my findings.

In my first attempt at differentiating my case load, I discarded the named states and took as my guide instead, some prominent clinical feature, such as the age of onset (early, middle or late), the rate of progress (acute, subacute or chronic), the amount of achieved or retained speech (mute, phrase or sentence), the duration of the illness (short, intermediate or long), the cognitive level (low-grade, high-grade or non-defective), and the amount of activity (hyperkinetic, normal or hypokinetic). The application of any one of these categorizations gave inconsistent results. The reason for this was that they were bound up with the vicissitudes of the psychotic process in its natural history. What passed for different diseases were different stages of the same disease. There appeared to be two basic patterns of development; one had an early onset and chronic course with little or no upheaval, and the other, of later onset, a course characterized sequentially by an initial, apparently normal or precocious phase, an episode of great turbulence subsiding into a withdrawn, regressed and rigid period, and finally, a phase of partial recovery at a lower level of functioning. With such a course, it is difficult to decide when psychosis begins or ends. For some authors, whatever follows the early upheaval is post-psychotic when compensations for the damage are being made, and they point to cases of shy, 'schizoid' behaviour in later childhood and adolescence where there is a history of mutism or turbulence in the first few years.

In my next attempt (Anthony, 1958), I tried to combine the eminent names with the clinical features. In Group I, typified by an early onset and a slow chronic course, I included Kanner's syndrome, Bender's first age-group, and Despert's 'no-onset' type. The differential diagnosis was from mental deficiency and deafness. In Group II, where there was an onset during the 3-5 age period and an acute course followed by regression, rigidity and retreat, I included Heller's disease, the De Sanctis and Weygandt dementias, Mahler's symbiotic psychosis, Bender's second age-group and Despert's 'acute-onset' type. The differential diagnosis was from the organic dementias. In Group III, characterized by late onset and a fluctuating subacute course, I included the neuropsychosis described by Ekstein and others, and Bender's third age-group. The differential diagnosis was from severe obsessional states. This classification was an improvement on the first, once again using the fitting of the data as the necessary requirement.

At this point I set about making a film summarizing the clinical aspects of childhood psychosis (Anthony, 1957*a*) and at the same time giving expression to my belief in the continuity of psychosis with the other diagnoses of child psychiatry. I now placed my list of eminent names in a new setting made up of five diagnostic continua ranging from a non-psychotic to a psychotic pole with an intermediate border-line condition. At the first non-psychotic pole I put the normal child; in the intermediate position along this continuum was the hypersensitive condition described by Bergman & Escalona (1949); and at the psychotic pole were the syndromes of Kanner and Mahler. At the second non-psychotic pole there was the mentally defective child; in the intermediate position, deficiency with a superimposed psychosis (Weygandt); and at the psychotic pole Bender's pseudo-defective psychosis in which the psychotic child functions unevenly at the defective level. At the third non-psychotic pole was the brain-damaged child; in the intermediate position, the child with a cerebral lesion with a super-

imposed psychosis; and at the psychotic pole the conditions named after Heller and De Sanctis. At the fourth non-psychotic pole was the neurotic child; in the intermediate position the child suffering from neuropsychosis; and at the psychotic pole, Bender's pseudo-neurotic psychosis and Ekstein's psychosis. At the fifth non-psychotic pole was the psychopathic child; in the intermediate position, one with psychopathy with a superimposed psychosis; and at the psychotic pole Bender's pseudo-psychopathic psychosis. In conjunction with these five continua—the normal, the defective, the organic, the neurotic and the psychopathic—I constructed a 5-point symptom-rating scale that could help to locate any particular case on its continuum. The thirty rateable items included genetic loading, sibling disturbance, degree of withdrawal, level of sensory threshold, amount of communicative speech, range of emotional expressiveness, extent of activity, degree of rigidity and stereotypy, and so on. Low scores pointed in favour of 'organicity', a loose term taken to include factors of constitution, inheritance and organic damage. High scores, on the other hand, were held to indicate the effects of psychosocial environmental determinants. Intermediate scores were thought to be suggestive of the borderline states. This supposed dichotomy into somatic and psychological was clearly a false one, since even the most superficial inspection of the case material revealed the workings of multiple causation. But it was also equally clear that the genetic, organic and environmental loading varied considerably from case to case, and, almost as if some principle of parsimony was involved, whenever one factor predominated the others tended to be minimal. In a few cases this did not seem to apply; nature then appeared to operate with what looked like unnecessary extravagance in its use of causal agents.

The distribution of cases according to social class revealed that 88 % of the 'organic' group belonged to the working class and 11 % to the professional, whereas 14 % of the 'environmental' group fell into the working class

and 43 % came from the professional class. On the basis of this first encouraging analysis, we decided to test the environmental hypothesis by three different methods—assessment of parental behaviour by means of the Fels Rating Scales, of maternal emotional responsiveness by the use of certain Rorschach criteria, and of child-rearing techniques by the administration of a special questionnaire, modified from Shoben.

The Fels scales consist of thirty variables that are selected on the basis of being observable, rateable, basic and universal. The background theory to the scales presupposes a consistency of parental behaviour from situation to situation so that a child, exposed to these consistent behaviour patterns over a period of time, eventually incorporates them into his personality. The degree of reliability of the Fels scales is unusually high for this type of measurement, approaching that of a well-designed psychometric test. (Carried out by E. Donnelly).

The two groups, the 'organic' and the 'environmental', were not differentiated to any significant extent by the Fels scales although such differences as there were, were in the expected direction. The 'organic' cases were shown to come from better-adjusted homes where there was more understanding, acceptance and rapport. The 'organic' siblings, too, were subject to less discord and received more affection than the 'environmental' siblings. They were more accepted and less criticized. The findings did not lend much support to the view that the 'environment' was psychotogenic.

The Rorschach analysis (carried out by Eva Bene) attempted to link the psychotic child's failure to form satisfactory human relationships to his failure in his first human relationship with his mother and ultimately to her inability to make satisfactory human contacts. According to Ainsworth & Klopfer (1954) this type of disability is inclined to reveal itself in the Rorschach by a tendency to turn human figures into animals and to give M responses with (A) content. Content with Hd (H), or (Hd) may

also indicate difficulties in interpersonal relationships.

The results showed that the 'environmental' mothers gave a higher proportion of MA, M(H), MHd and M(Hd) responses and of responses indicating various kinds of disturbing phantasies than did the 'organic' mothers. The differences seemed reasonably large and in the predicted direction, although significances could not be calculated because the data were not independent. The results, however, were rendered questionable by the fact that the two groups of mothers were not at all comparable from the point of view of intelligence. The majority of the 'organic' mothers came from the working-class and were dull to normal in intelligence; the majority of the 'environmental' mothers were from the professional classes and tended towards a superior level of intelligence. The mothers in the two groups also differed in the length of record they produced. Even allowing for these two vitiating factors, there did appear to be a tendency over and above the question of intelligence and length of record for the 'environmental' mothers to show less Rorschach capacity to form satisfactory human relationships, but the findings, again, were not sufficiently striking to lend any conclusive support to the environmental hypothesis. A later study by Bene (1958) in which the mothers of children with primary and secondary autism were compared, has yielded interesting results.

The questionnaire dealing with child-rearing behaviour and attitudes did not help very much in this matter. The fifty-one items covered areas of discipline, sexuality, aggression, etc., but the responses failed to differentiate the two groups. There was, however, one additional feature to the questionnaire which was fairly enlightening. When the mothers noted their behaviour with regard to any particular item, they were, at the same time, asked to state whether they felt that what they did or were doing was right or wrong or whether they were uncertain about one or the other. The results showed that the mothers tended to use one of these three comments on

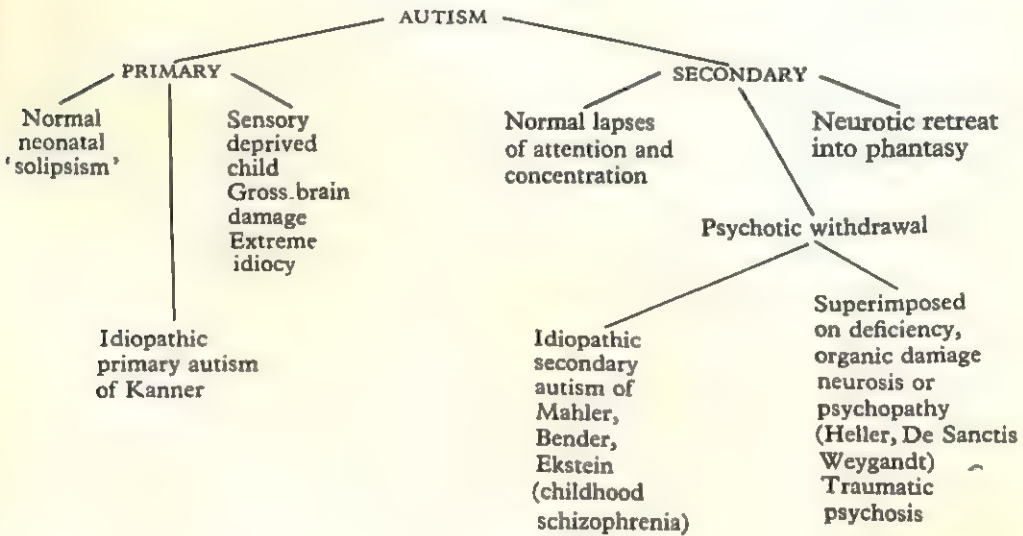
their own behaviour more frequently than the other two. One could therefore distinguish three types—a 'righteous', a 'guilty' and a 'perplexed'. Unfortunately, these three types seemed to bear little relation to the 'organic' and 'environmental' groups.

Continuing the search for a more satisfactory correspondence with the data, I turned to one of the most striking symptoms of the psychotic child—his autism, the 'qualifying adjective of childhood psychosis' and investigated its potentiality as a diagnostic criterion.

ness to remoteness. Every infant begins its psychological life in an autistic state, and from this it gradually emerges as a result of constant environmental stimulation. Occasionally the child fails to emerge from this infantile autism; occasionally, having emerged, it may retreat under conditions of stress back into the autistic state. There are therefore two types of infantile autism (Table 2).

In about a third of the primary cases, there is a gradual emergence from the autistic cocoon. Non-emergence is associated with a

Table 2



There are several components to this phenomenon—an *a-cathexis* or inability to form human relationships; an *a-dualism*, to use Piaget's term, indicating an inability to differentiate self from non-self, internal from external and subject from object, so that egocentricity tends to persist and identity fails to emerge; and finally, an *a-genesis* of certain maturing psychological functions leading to inadequate communicative and expressive abilities and an increase in autistic behavioural patterns.

Autism plays a part in both normal and abnormal living. It can occur transiently, sporadically, intermittently, fluctuatingly and persistently. It can vary in degree from aloof-

failure to develop communicative speech. The child can be regarded as grossly understimulated, either because he is not adequately equipped to receive stimuli, or because the stimuli are unable to get through to him, or because the environment—and this means chiefly his mother—has a deficient stimulating capacity.

In secondary cases, the reverse is true. The child is grossly overstimulated at first, either because he is too receptive or too sensitive or because the environment is excessively stimulating. His mother tends to smother him.

In the primary cases, therefore, there is a combination of unresponsive child and unresponsive mother only in the peculiar idio-

pathic group described by Kanner. Primary unresponsiveness in the child (Erikson refers to it as 'sending power') when matched with an unresponsive mother can produce a degree of autism of psychotic intensity. Given reasonable mothering many of these primary cases, especially those with sensory deprivation, will gradually overcome this initial handicap. In secondary cases, the over-responding, hypersensitive child meets with an overstimulating mother or a mother deficient in protective

To understand these psychotic reversals, one can make use of the Barrier Hypothesis, first postulated by Freud (1922) and used as an explanatory concept by Bergman & Escalona (1949). According to this, interposed between every organism and its environment (internal or external) is a stimulus barrier or *Reizschutz*. Like every other piece of human apparatus, the barrier has a history, normal and abnormal, the facts of which are summarized schematically in Fig. 1.

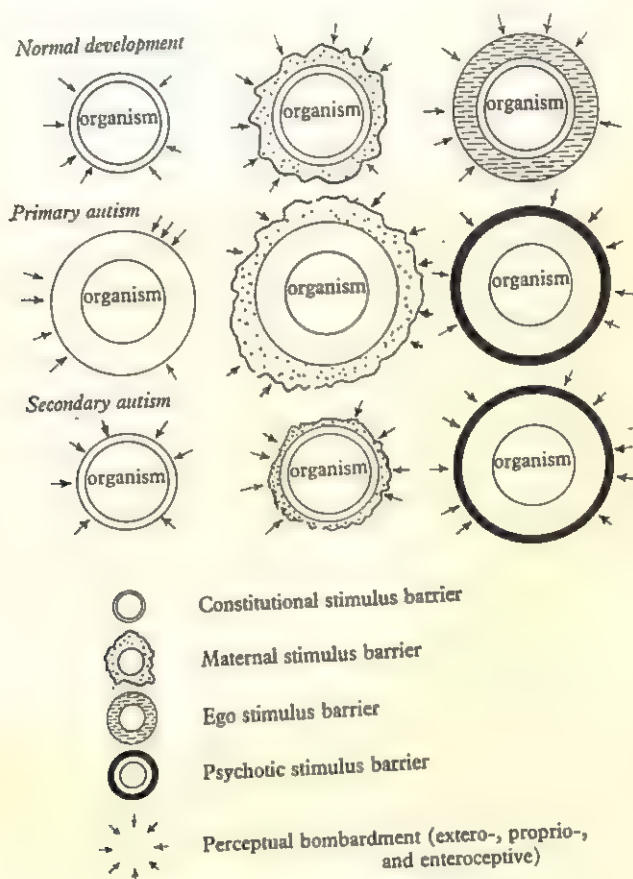


Fig. 1. The barrier hypothesis

capacity and the result again is a degree of autism of psychotic intensity. In one therefore the child fails to emerge; in the other he emerges only to retreat. In the former, the hypo-sensitive child gradually may become sensitive; in the latter, the hypersensitive may suddenly or gradually become insensitive.

During the course of normal development, the primary or constitutional barrier which protects the newborn human organism and has little but crude survival value is supplemented by the maternal barrier. This is based on the loving care of an understanding mother, and its efficiency varies from an intuitive and

apparently instinctive buffering of all but tolerable stimuli to gross ineptitude. As long as the child is normal, it seems capable of surviving maternal inefficiency without a clinically increased shock sensitivity. The maternal barrier is eventually replaced by an autonomous ego barrier, modelled on it but far more selective in its operation. The ego barrier begins to work at the time when the child first leaves the mother's immediate orbit, and allows the organism to focus its attention and concentration against the distraction of the perceptual bombardment. The child is in fact not ready to leave the maternal orbit until the self-protective ego barrier is functioning, and its function is intimately bound up with an intact cerebrum. The brain-damaged child thus becomes highly distractible and is at the mercy of every passing stimulus.

Where the organism is constitutionally abnormal, the maternal factor becomes critical and the development of the autonomous barrier may be disturbed, delayed or absent, and replaced by some pathological structure.

psychotic barrier with the result that he fails to emerge from his primary autism.

In cases of secondary autism, the constitutional barrier can be regarded as abnormally 'thin' so that an excessive amount of stimulation gets through, and the child becomes hypersensitive and over-reactive. Not only is the mother unable to prevent this, but she adds to the bombardment by her own uncontrolled stimulations. Eventually, the child withdraws behind a thick, unselective psychotic barrier which blankets the stimulation. Sometimes, as in the so-called 'under-cover child' (Anthony, 1957*a*), he may even reinforce the barrier with various types of material covering. These indicate an inadequate psychotic barrier, and without them he seems curiously vulnerable—a 'creature without a shell'.

* * * *

This simplest of diagnostic subdivisions into primary and secondary autism fitted our clinical and experimental data more satisfactorily than any of the other classifications.

Table 3

Speech capacity		Primary (%)	Secondary (%)	Regression to autism ↑
↓ Progression from autism	Mute	75	8	
	Phrase	15	42	
	Sentence	10	50	

In cases of idiopathic primary autism, the constitutional barrier can be regarded as abnormally 'thick'. The mothers frequently allege that their diminished contact and stimulation of the child is secondary to his primary unresponsiveness, but clinical investigation invariably discloses their own concomitant lack of sensitivity and responsiveness over and above the 'thick skin' of the child. Where the mothers are normal and the children suffer from some sensory deprivation, the challenge of autism is met and mastered. In the absence of normal maternal stimulation, the child's constitutional barrier becomes an unselective

I will deal first with the clinical fit, and begin with the speech categories.

Speech is generally recognized as one of the most favourable prognostic signs in childhood psychosis. In terms of the named syndromes, the natural history, or the organic-environmental dichotomy, it was difficult to understand why this should be so. The distribution of speech through the sample appeared haphazard and unrelated to the clinical state, except perhaps, to the level of intelligence. This is difficult to gauge in any psychotic child, but almost wholly unreliable in the mute one. There is a better relationship to the social

quotient (S.Q.) as assessed on the Vineland scale, but the correspondence is not a revealing one.

It will be seen from Table 3 that there is a progressive sequence in the primary group, indicating, perhaps, the increasing difficulty in learning and developing new skills, especially social skills, in the face of infantile autism. The developmental distance encompassed by the child may depend on the presence of a number of different factors—the initial intensity of autism, the genetic tendency towards autism, the amount of stimulation derived from the environment (especially the presence of normal parents and siblings), the level of the sensory thresholds and the presence or absence of organic damage to the brain. In some cases of congenital aphasia that I have seen, a normal mother and a stimulating environment have helped to break through the autism within the first five years, so that children who were initially referred as psychotic were later observed to make good contact and to develop a normal curiosity about their environment.

The secondary group shows a regressive sequence towards a pre-verbal state of infantile autism. The developmental distance travelled in this backward movement may depend on the presence and degree of an infantilizing tendency in the mothers, a traumatic level of stimulation from the environment, organic damage to the brain and again, the genetic tendency towards autism. Backward development into infantile autism is not often complete unless there is gross cerebral disease. A case I have observed was referred as incipient psychosis. In the course of two months we were able to witness a regressive sequence of increasing autism associated with gradual loss of sentences, then of phrases and finally of words. A state of complete autism with only a neonatal degree of responsiveness was the end result. The condition was diagnosed, when neurological phenomena eventually made their appearance, as one of sclerosing leuco-encephalitis and the diagnosis was confirmed post mortem.

Table 4 shows the distribution of cases according to socio-economic class divisions.

Table 4

	Social classes (%)				
	I	II	III	IV	V
General clinic*	4	12	55	16	13
All psychosis	29	25	22	22	2
Primary group	43	23	20	14	0
Secondary group	9	20	40	19	12

* Figures obtained from the Triennial Statistical Report issued by the Bethlem Royal and Maudsley Hospitals.

It will be observed that while the secondary cases closely follow the general clinic trend, the primary cases show a predominant incidence in the upper social groups. This may help to explain some of the conflicting findings hitherto reported. Kanner has pointed to the good social and professional status of the parents of his autistic group of children, while other workers have insisted that no special tendency of this kind seems to exist. The reason for this clustering of the primary group will be commented on later.

The mental status of the parents has been discussed, but here I am concerned with a clinical rating along scales of maturity, passivity, dominance and introversion (? the adult equivalent of autism). As in many clinical assessments, the category of normality was hard to use. None of the parents were classified as 'normal', but three were 'doubtfully normal'. It may be that one cannot be clinically normal if one has a psychotic child; either one is abnormal and understandably gives birth to an abnormal child, or an abnormal child understandably forces abnormal patterns of behaviour on to its mother, or one is examined by a psychologist with a nurture or nature bias. The dimensions grew out of experience, but even when fully defined produced inadequate profiles that were not easily recognizable by a second judge. About half the primary fathers tended to be weakly passive, and a

quarter introverted obsessives; the secondary fathers were mostly weakly passive. The mothers in both groups were domineering, rigid, controlling and criticizing (60-70%), but about 40 % of the secondary mothers were immature and perplexed. This analysis did not tell us much, and psychiatric categories (normal, neurotic, psychopathic, psychotic) even less.

Taking the self-criticism part of the parental questionnaire previously mentioned, and analysing the 51 responses in terms of 'self-righteous' (40+ rights), 'guilty' (10+ wrongs) and 'perplexed' (15+ uncertainties), we found that 50 % fell into the 'self-righteous' category, 39 % into the 'perplexed', and 11 % into the 'guilty' class. This is not significantly different from the responses of a batch of mothers with neurotic children, although the mothers of psychotic children tend to give the more extreme responses in each category. But if we now look at these same results in terms of primary and secondary autism, we obtain the results shown in Table 5.

preoccupied with the dreadful thought that something that they have done may have led to the disastrous result.

The actual child-rearing methods of the two groups are not so very different from each other or from those used by the mothers of neurotic children, which once again underlines the truism that it is not what you do that is likely to affect the child; it is the feeling with which you do it. The parental questionnaires demonstrate this clearly. The mothers in both groups seem to lack 'an instinct for mothering'; the group of primary mothers compensates in a typical middle-class way—it turns to the books. ('I did it according to the book; I pride myself that I did not put a foot wrong in his upbringing.') The book is always right. There is therefore a tendency for primary mothers to be 'self-righteous' and for 'self-righteous' mothers to be middle-class. This does not, of course, in any way explain the middle-class preponderance of the primary mothers, but it is an interesting commentary on the fact.

Table 5

Type of autism	'Self-righteous' (%)	'Guilty' (%)	'Perplexed' (%)
Primary	81	2	17
Secondary	15	16	69

It will be observed that the 'self-righteous' are predominantly of the primary group. Are they lacking in insight or is there some other explanation? They have produced a 'bad' child, and yet they do not feel guilty. This may seem mysterious until we remember that their children are 'primary' children. These children have never been normal. Therefore something is clearly wrong with the children and not with their mothers' care. The mothers have nothing to feel guilty about nor, for the same reason, are they driven to have doubts about their techniques.

The secondary group of mothers is quite different. The mothers are both guilty and confused, and tend to show neurotic reactions to the psychoses in their children. They are

The mental status of parents was examined clinically in terms of their ability to 'make rapport' with their children, with other ward children, and with members of the staff. They were classified, in the context of responsiveness, into cold, lukewarm, warm, and smothering emotional types. There was a good measure of agreement among the judges, who found that 73 % of the cold mothers belonged to the primary group and 62 % of the smothering mothers fell into the secondary group. This would be in accordance with the barrier hypothesis.

One further interesting clinical feature also emerged on further analysis. There was a predominance of first-born children in the primary group, suggesting the possibility of birth

trauma as a cause of primary unresponsiveness. These idiopathic primary cases might therefore find a place somewhere on the continuum of reproductive casualty (Pasamanick, 1954).

* * * *

We come, finally, to an experimental appraisal of the phenomenon of autism. In the groups under investigation the cases were too few in number to receive statistical treatment. The different projects must be regarded as pilot undertakings leading perhaps to further, fuller inquiries. The experiments are also useful in telling us the ways in which psychotic children behave when set in a controlled situation. The interpretation of the results must be regarded as largely speculative.

(1) *Investigation into the nature of the psychotic barrier*

The psychotic barrier, as postulated in the barrier hypothesis, can be theoretically assessed in both qualitative and quantitative terms. Qualitatively, the response to different types of sensory stimulation by the organism can be seen in hierarchical or selective arrangements. Hierarchically, responsiveness may range from a mild degree of inattention through visual avoidance, auditory imperception, blunting of pain to loss of tickle sense. At times the responsiveness may be more selective, so that the organism tends to suppress only one modality and is referred as a case of specific sensory impairment. Psychotic children are constantly being investigated for special forms of deafness. The quantitative assessment of barrier penetrability may be made in terms of sensory thresholds, the easiest to explore in the case of psychosis being that of sound. The crucial problem in these investigations lay in finding out to what extent the psychotic barrier had a physiological or psychological basis.

This is how it was studied. The children were manoeuvred into a small prefixed circle on the ground so that their backs were towards the source of sound, in this case the dropping of a series of weights at a fixed distance from the

circle. (Only those who have dealt with psychotic children will realize the infinite difficulties entailed in the word 'manoeuvred'!) The children were rated on a scale at one end of which there was a startle response, followed by a bodily movement to or away from the source of sound, the turning of the head to or away from the source of sound, a change of facial expression indicating awareness, and finally, no observable reaction. (It was not possible to use a P.G.R. apparatus on these cases.) The results, when repeated, were so variable that they could lead to one conclusion only—the barrier was a psychological one. The experiment was then modified. The graduated series was replaced by a single loud sound, and the test was repeated every day for eight days. The psychotic children were matched against neurotic ward children for age and sex. The psychotics were all mute or phrase children.

To summarize the results. The psychotic children showed no startle response at any time. In marked contrast to the neurotic controls, they showed no preparatory set after the first experience. They invariably turned away from the source of sound and not towards it. They showed a rapid adaptation to the situation, and lastly, gave a large number of zero or 'no' responses (these were given by no control cases). Primary cases adapted quicker and had more zero responses.

Leading on from this small project, I attempted to investigate the nature of the perceptual discrimination involved. I used a piece of apparatus constructed for social conditioning and modelled on that of Smolensky (1927). The unconditioned stimulus was a chocolate ball that ran down a trough and presented itself alluringly on a small stage facing the child. For conditioning stimulus I used a flash of light and a buzzer. The reaching behaviour of the child was recorded on a continuously revolving drum by the breaking of three photo-electric circuits.

After some initial hesitation, all the psychotic children reached for the chocolate as soon as they saw it appear. I compared them

with a small group of 2 to 3-year-olds who had roughly the same mental age. The results were as follows: none of the psychotic children could be conditioned with a light stimulus; there was some conditioning with sound, but less than with the controls; lack of reinforcement brought about quicker extinction in the psychotic group, and thereafter the tendency seemed completely and permanently lost (checked after a period of three months)—they could never be conditioned again in this particular situation and with this type of conditioning. Primary cases were more difficult to condition and easier to extinguish than secondary cases.

Judging from these two small experiences, there is a suggestion that among many other possible factors it is the rapid adaptation to the unexpected stimulus, combined with the rapid extinction of the unrewarding stimulus, that helps to dissociate the psychotic child from its environment and thereby sustains its autism. The results also suggest that the primary barrier is, on the whole, more difficult to penetrate than the secondary barrier. This fact may be linked with the somewhat poorer prognosis in the primary cases.

(2) *Investigation into the regression of the object concept*

Having studied the reaching behaviour of the psychotic child, we turned next to the nature of his searching behaviour—the way in which he looks for objects (Anthony, 1957*b*).

There are two basic ways in which the organism adapts itself to the object world that surrounds it. First, it gradually learns to know the object and to recognize its manifold quantitative and qualitative characteristics. This has been termed the cognitive orientation. Secondly, it gradually learns to relate to the object and develops feelings that link it with the object in an endless interaction. This is the cathectic orientation. Both approaches are essential to a full experience of the object world, but under different circumstances and in different individuals one or other may predominate. An imbalance of this sort is

especially likely to occur in clinical states, above all in psychotic ones. As a result of psychosis, the feeling between the subject and object is lost (*a*-cathexis), and the distinctions between the two become indeterminate (*a*-dualism) so that the world inside fuses with the world outside. When this is coupled with regression and rigidity, a simplification of object behaviour results, causing the psychotic child (I refer here to secondary cases) to slide backward down the hierarchical scale of objects from structured to unstructured, until it eventually reaches a level at which things lose all meaning except as part of some primitive sensory-motor patterning.

In this investigation a scale of development of the object concept modified from Piaget was used (Table 6).

All these assessments of object behaviour are bound up with one of the essential ingredients of autism—*a*-dualism. To the extent to which the subject is unable to differentiate itself clearly from the object, to that degree will object behaviour be disturbed or distorted. Piaget, although emphasizing the sequence of the stages, does give approximate ages for the mastering of these developmental tasks, but mental age is probably the closer guide (Russell & Dennis, 1939). The test scores ranging from 4 to 1 represent complete, incomplete and absent notion of a particular object concept, and therefore a hierarchical analysis. Each psychotic child was given an 'organismic' age (Olson, 1957) derived from its chronological age, its mental age, its social age (Vineland) and its bone age. The age period covered by the series of developmental tests runs from 0 to 12 years. When the ages indicated by the levels of test behaviour are plotted against the 'organismic' age, the resulting scatter diagram shows some interesting features.

(1) When the psychotic group is treated as a whole, object behaviour follows cognitively (but not cathectically) the reactions of a normal infant ($r=0.24$). When, however, the main psychotic group is split into the primary and secondary components, the correlation with

Table 6

Searching behaviour
(Whole object)

1. No search for object.
2. Unscreening of object.
3. Search at point of disappearance
4. Search through successive displacements

Object constancy

1. No notion of constancy
2. Size but not shape constancy
3. Shape constancy
4. Colour constancy

Object name

1. Names transferred, generalized and confused
2. Name intrinsic part of object
3. Name partly in object, partly in mind
4. Name understood as linguistic convention

Searching behaviour
(Part object)

1. No investigation.
2. Investigation with accidental extraction—not repeated
3. Accidental extraction followed by purposeful extraction
4. Immediate purposeful extraction

Object conservation

1. No notion of conservation
2. Conservation of matter
3. Conservation of weight
4. Conservation of volume

Object animism

1. Every object alive, conscious and sensitive
2. Only objects that move have life, consciousness and sensitivity
3. Only objects that move by themselves have these
4. Only biological objects have life, etc.

regard to the primary cases increases ($r=0.65$), whilst the correlation with regard to the secondary cases decreases ($r=0.19$). It would seem therefore that the forward development of the primary children coincides with the forward development of normal children, whereas the backward development of the secondary child is much more a perversion of human behaviour.

(3) *Investigation into the egocentricity of the psychotic child*

For this project we made use of a little test devised by Piaget called 'The Test of the Three Mountains'. This consisted of a series of toy mountains of different configuration and height. A toy child climbed the mountains one after another. At the top of each he took a photograph of the landscape in front of him. These photographs we presented to the children, asking them to identify the position from which the 'snap' was taken. In order to be able to do so the subject must be able to put himself in the position of the perceiving toy child and

see things from his point of view. Piaget found that children under the age of seven found it difficult to exchange their subjective perspective and he termed this the child's 'egocentricity'. When one comes to analyse the phenomenon, one sees that it has several different components. It contains, for example, affective features somewhat akin to Freudian narcissism, cognitive features giving rise to 'infantile realism' (Piaget, 1929), moral features constituting 'moral realism' (Piaget, 1932), and finally, perceptual features, with which we were mainly concerned in this test. All these distortions of the subject-object relationship belong together and represent a particular era in childhood during which certain autistic phenomena are predominant. A-dualism and egocentricity are both manifestations of autism.

It was therefore predicted that psychotic children would score highly on this perceptual test for egocentricity. We took a group of fifteen 'sentence' psychotic children from our

sample, with ages ranging from 8 to 12 years and intelligence scores from I.Q. 70 to 90 and matched them against fifteen neurotic clinic children for age and I.Q.

The toy child was fixed on the second of three mountains. Four responses were possible, so that snap 1 (from the position of the subject) indicated the highest degree of egocentricity; snaps 2 and 4 (mountains 1 and 3) indicated a lower degree of egocentricity; snap 3 (mountain 2) indicated a normal response.

It was realised that chance might govern the selection in some cases, and so the test was repeated after 7 days. Correlations for the test-retest scores were 0.92 for the neurotic group and 0.47 for the psychotics. The group results were as shown in Table 7.

Table 7

	Normal (%)	Low ego (%)	High ego (%)
Psychotic	35	38	27
Neurotic	87	7	6

$N=15$

The percentage responses show a marked difference in the capacity of the psychotic and neurotic children for a normal perspective. Many of these children were rather dull, and one would expect that in this age range normal children of average intelligence would give near to 100 % correct responses. There was some chance factor in selection among the psychotics, but it should be noted that those among them who gave correct responses in the first test almost invariably gave correct responses in the retest. Unfortunately the group was too small to split up into primary and secondary cases.

* * * *

The diagnostic problems discussed in this paper are closely related to the fact that no one yet has been able to follow the whole course of a psychotic child from earliest infancy to adolescence and after. So far we have been in the position of making retrospective and prospective inferences from cross-

sectional data. Are there any ways open to us to make crucial longitudinal studies in the future? Can we anticipate the development of a psychosis in a child?

There are four possibilities that we can examine:

(1) If we believe that the condition is genetically determined, and that psychosis abounds in the families if not in the parents of the psychotic child, then we might follow a small sample of parents and children with a heavy psychotic 'tainting'. I do not think, from experience, that this has much hope of success.

(2) If we believe that the parents of psychotic children are 'psychotogenic' and that we are able to recognize this state in the parents, we might again be able to follow a sample of such parents through their reproductive lives. I do not think that we could distinguish such parents when we saw them unless we knew that they had a psychotic child. It might be a worthwhile and sobering experience to expose clinicians to a blind judgement of a group of mothers containing parents of psychotic children to see if they can spot them.

(3) If we believe in the effects of severe environmental factors, we might be able to follow a group or highly traumatized children. I do not think that the traumata which sometimes seem to precipitate a psychosis in childhood are anything greater than normal developmental hazards (sibling birth, etc.). It is the predisposition that makes them vulnerable.

(4) If we believe (in the secondary cases) in a well-defined pre-psychotic phase characterized by enhanced or unusual functioning in the motor-autonomic sphere (Bender) or in the sensory sphere (Bergman, Escalona), it might be possible to select a group of these cases (after we have refined our diagnostic criteria) and investigate them intensively. I consider this the most feasible of all the possibilities. After a preliminary experience of this nature, we might be able some day to conduct a predictive longitudinal study and verify or refute the many 'hunches' that have been set out in this paper.

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A RORSCHACH INVESTIGATION INTO THE MOTHERS OF AUTISTIC CHILDREN

By EVA BENE*

In the paper by Anthony (1958) on an experimental approach to infantile autism reference is made to the investigation of mothers of psychotic children with the Rorschach technique. For practical reasons Rorschach tests could only be given to the first thirty-eight mothers studied by Anthony, so that not all of his sample was used in the present investigation.

The Rorschach records were used to test two hypotheses set up by Anthony regarding personality differences between mothers of children with primary, and mothers of children with secondary autism. The two hypotheses may be formulated as follows:

1. The mothers of children with primary autism are less frequently able to establish social and emotional relationships than are the mothers of children with secondary autism.

Table 1. *Differences between mothers of children with primary and secondary autism in relation to (1) their ability to establish social and emotional relationships, and (2) the morbidity of their phantasy life, as indicated by their Rorschach responses*

	Mothers of children with primary autism (N=21; mean R=25)	Mothers of children with secondary autism (N=17; mean R=26)	Level of significance (one-tailed test)
Hypothesis 1			
Less than 2 H responses given by	11	2	0.005
Less than 2 M responses	11	2	0.005
Less than 2 Fc responses	15	9	0.10
Less than 2 FC, CF responses	8	2	0.10
Hypothesis 2			
More than 2 morbid responses (2=median number of morbid responses of combined groups)	6	9	Not significant

2. The mothers of children with secondary autism more frequently have a morbid phantasy life than have the mothers of children with primary autism.

* Manuscript received 20 June 1958.

METHOD

The first step in this investigation was the setting up of Rorschach criteria by which the two groups of mothers could be compared in relation to Anthony's hypotheses. The following criteria were taken as indications that a mother had difficulties in establishing social and emotional relationships:

1. She gave less than 2 H responses;
2. She gave less than 2 M responses;
3. She gave less than 2 Fc responses;
4. She gave less than 2 FC, CF responses.

A morbid phantasy life was taken to be indicated by morbid responses, such as witches, skeletons, blood, storm clouds, dangerous fires, icebergs; and by seeing humans, animals or things that were damaged, diseased, in

trouble, falling, hanging on, balancing, killed ugly, repulsive, nasty, grotesque, frightening or threatening. It was decided to test the second hypothesis by establishing the number of mothers in each group who gave a greater

number of such responses than was the median of the combined sample.

Each record was scored on the basis of these criteria, without the investigator knowing from which of the two groups of mothers it had come.

RESULTS

Anthony's first hypothesis was substantiated by the results. It can be seen from Table 1 that all four criteria yielded significant differences in the predicted direction. This indicates that mothers of children with primary autism less frequently show the selected Rorschach signs of capacity for establishing social and emotional relationships than do mothers of children with secondary autism.

Anthony's second hypothesis was not confirmed by the results. No significant difference was found between the mothers of children with primary and the mothers of children with secondary autism regarding the morbidity of their phantasy life, although the difference between them does go in the predicted direction. Expressed in other terms than those of Table 1 the results show that 53 % of mothers of children with secondary autism gave more than two morbid responses to the Rorschach, compared with 29 % of the mothers of children with primary autism. In all cases the Fisher-Yates test of significance for small samples was used.

REFERENCE

- ANTHONY, J. (1958). An experimental approach to the psychopathology of childhood: autism. *Brit. J. Med. Psychol.* 31, Parts 3 and 4, 211-25.

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EYSENCK'S PERSONALITY THEORY WITH SPECIAL REFERENCE TO 'THE DYNAMICS OF ANXIETY AND HYSTERIA'

By LOWELL H. STORMS AND JOHN J. SIGAL

However valid might be the criticisms made in previous discussions of Eysenck's psychology of politics (Christie, 1956*a, b*; Eysenck, 1956*c, d*; Hanley & Rokeach, 1956; Rokeach & Hanley, 1956) or of his views on psychotherapy (Eysenck, 1954, 1955*c*; Luborsky, 1954; Rosenzweig, 1954), they are somewhat peripheral to his theory of personality. The most thoroughly elaborated and extensively investigated portion of this theory is the subject of his recent book, *The Dynamics of Anxiety and Hysteria* (1957), the publication of which followed not far behind a preparatory discussion of the theory (Eysenck, 1955*a, b*).

If this new set of hypotheses can stand the test of critical examination, one of the long sought after keys enabling personality studies to be linked to work in academic experimental psychology will have been found, and exciting new possibilities will be presented to clinician and laboratory worker alike. Although Eysenck points out that his theory does not cover the whole realm of behaviour and allows mostly qualitative rather than quantitative deductions, his summary evaluation is that '... the number of facts which support some such theory as that put forward here is so large by now that it cannot be dismissed out of hand' (1957, p. 257), and he feels that the implications of

'... the results of our work (to the relationship between psychological theory and psychiatric practice) are quite drastic' (p. 250). He has also thrown down the gauntlet with the words: '... when all is said and done, the critic who wishes to dismiss the whole of the theory would have to explain in terms of some other theory the observed experimental data... the reader might try to deduce the reported facts relating to satiation, conditioning, reminiscence, spiral after-effect, drug effects, brain damage, and so forth from some such theory as the psycho-analytic' (p. 257).

It is not our aim to accept this formidable challenge. We intend, rather, to examine some of the 'reported facts' in the light of deductions made from Eysenck's theory. In so doing, we hope to present the reader with a more balanced picture of the empirical support for the theory than can be obtained by reading the book alone.

An imposing array of experimental and anecdotal evidence is presented in the book and, with but few exceptions, interpreted as being predictable from the theory. In order to deal with work deemed relevant to the theory by its author, we have based our discussion almost exclusively upon studies cited in *The Dynamics of Anxiety and Hysteria* as favouring or being consistent with the theory set forth therein. This paper will present aspects of the same research unfavourable to the theory. Because the book ranges over many diverse areas of psychological research, some of them outside the limits of our competence, disproportionate emphasis may be placed on some topics and others may be largely neglected in this discussion. We refer the reader to the book, other writings of its author, and references given by him, to complete the picture.

* This paper was prepared during the tenure of a Post-doctoral Overseas Fellowship granted to J. J. Sigal by the National Research Council of Canada and a Fulbright award granted to L. H. Storms. Both were at the Institute of Psychiatry, Maudsley Hospital, University of London, during the year 1956-7. Dr Storms is now at the University of California (Los Angeles) Medical Centre, and Dr Sigal at the Hampstead Child Therapy Clinic, London. Manuscript received 3 July 1958.

THE THEORY

Eysenck sets forth his theory in two principal postulates, the *Postulate of Individual Differences* and the *Typological Postulate*, and extends its applicability with a drug postulate and a brain damage postulate. Hull's learning theory with slight modifications (p. 58) provides the remainder of the basic principles.

The Postulate of Individual Differences states: 'Human beings differ with respect to the speed with which excitation and inhibition are produced, the strength of the excitation and inhibition produced, and the speed with which inhibition is dissipated. These differences are properties of the physical structures involved in making stimulus-response connections' (p. 114).

The Typological Postulate states:

Individuals in whom excitatory potential is generated slowly and in whom excitatory potentials so generated are relatively weak, are thereby predisposed to develop extraverted patterns of behaviour and to develop hysterical-psychopathic disorders in cases of neurotic breakdown; individuals in whom excitatory potential is generated quickly and in whom excitatory potentials so generated are strong, are thereby predisposed to develop introverted patterns of behaviour and to develop dysthymic disorders in case of neurotic breakdown. Similarly, individuals in whom reactive inhibition is dissipated quickly, in whom strong reactive inhibitions are generated, and in whom reactive inhibition is dissipated slowly, are thereby predisposed to develop extraverted patterns of behaviour and to develop hysterical-psychopathic disorders in case of neurotic breakdown; conversely, individuals in whom reactive inhibition is developed slowly, in whom weak reactive inhibitions are generated, and in whom reactive inhibition is dissipated quickly, are thereby predisposed to develop introverted patterns of behaviour and to develop dysthymic disorders in case of neurotic breakdown (1957, p. 114).

One additional postulate says 'Brain damage leads to an increase in inhibitory potential, both temporal and spatial' (1957, p. 145), while the other states, 'Depressant drugs increase cortical inhibition, decrease cortical excitation, and thereby produce extraverted

behaviour patterns. Stimulant drugs decrease cortical inhibition, increase cortical excitation, and thereby produce introverted behaviour patterns' (p. 229).

The parts of these postulates referring to inhibition are virtually identical to the postulates advanced in a previous publication (Eysenck, 1955*b*). The parts referring to excitation have been added and are new. An important feature of these postulates is that they state that the same individuals develop inhibition quickly, develop strong reactive inhibitions, dissipate inhibition slowly, and are predisposed to develop extraverted patterns of behaviour. The converse is held to be true of other individuals, and it seems to be implied that those who are fast in developing inhibition are slow in developing excitatory potential and vice versa. (Whether this meaning is intended might well be clarified.) While the postulated correspondence between these tendencies is amenable to empirical investigation, which would involve applying measures of several phenomena to the same individuals, such a straightforward investigation of the relationships which he postulates is nowhere reported by Eysenck. The book is devoted to less direct, though intriguing, 'deductions' from his theory.

DIMENSIONAL BASIS AND GENERAL COMMENTS

Before presenting his postulates and going on to consider some of the relevant research, Eysenck prepares the way in three chapters called 'The Dimensional Analysis of Anxiety and Hysteria', 'Learning Theory and Human Behaviour', and 'Learning Theory and Personality: Critical'. A few comments regarding this groundwork seem to be in order.

Since his first chapter '... is largely a review of previous work, no effort will be made to prove in detail the various points made; for such proofs the reader is referred to previous publications' (p. 9). The present writers have read these 'proofs' and have found what they consider certain general deficiencies therein, as follows:

(1) *Failure to cross-validate before con-*

sidering a measure useful or adequate. An example of the relevance of this is the Eysenck & Prell study (1951) of the inheritance of neuroticism. In this study, the only measures with appreciable loadings on 'neuroticism' were tests of body sway suggestibility, static ataxia, and autokinetic movement. Only the first two of these tests have been used in later relevant research, and they have not held up as discriminators of neurotics from normals (S. B. G. Eysenck, 1955; Hildebrand, 1953). Another study of a similar nature was done by a student of Eysenck's (Blewett, 1953), who used different measures and found little evidence that neuroticism is inherited. The factor scores, heavily weighted with body sway, 'showed no agreement with ratings of neuroticism'. A second example is provided by Eysenck's use of unvalidated questionnaire tests of introversion-extraversion (I-E),* which will be discussed shortly.

(2) *Frequent failure to mention relevant test or population characteristics when making a case for the practical utility of his measures or methods.* There is notably a failure to mention test-retest reliabilities, which are known to be very low for some tests such as eye-blink conditioning (Franks, 1954), and a failure to consider base rates, which are of great importance in evaluating the effectiveness of screening tests for any purpose. An example of both of these omissions is provided by Eysenck's article reporting a canonical variate analysis of perceptual measures using normal, neurotic, and psychotic subjects (1955d).

(3) *Occasional failure to report relevant aspects of the method of test selection.* The article just mentioned is quoted at length in

chapter I of the book and will serve well as an example. In the original article, Eysenck referred to a monograph not then published (Eysenck, Granger & Brengelmann, 1957), but did not mention that other tests than the four described were given to the same subjects; in the book he says only that the four tests 'among others' were given. In fact, 77 scores from about 55 tests were obtained for each subject. If the four tests were selected according to whether their patterns of group means suggested two dimensions rather than one, the appropriateness of the significance test which Eysenck reports is in doubt, for it assumes random or arbitrary selection of the tests involved. The giving of 55 tests makes possible a large number of chance permutations and combinations, some of which would yield two 'significant' dimensions.

The same study has been criticized on other grounds by Jenkins & Lykken (1957), Loevinger (1955, see Eysenck, 1956a), and Storms (1958). Jenkins & Lykken have shown, for example, that while qualitatively different physical disease groups can be placed on continua with respect to certain measurements, as can be done by Eysenck's canonical variate analysis, the continua may be quite irrelevant to the classification, understanding, or explanation of the diseases themselves.

Discussing a similar canonical variate analysis reported by S. B. G. Eysenck, H. J. Eysenck says she gave six tests (the ones used in the analysis he reports), whereas she actually gave 20 different tests yielding 38 scores for each subject. S. B. G. Eysenck lists as criteria for selecting the six scores significant F ratios, only one score from each test, and homogeneity of variances, among others. However, one of the six selected scores yielded heterogeneous variances ($F = 3.74$, $p < 0.01$ when the variance for neurotics is compared with that of the normals). The fact that two other scores from the same test did satisfy all her criteria (if they had been selected, the analysis might not have supported Eysenck's contentions) suggests that selection was not random within the specifications of the criteria.

* The following abbreviations are used throughout this paper: MMPI (Minnesota Multiphasic Personality Inventory); MMQ (Maudsley Medical Questionnaire); I-E (Introversion-Extraversion); MPI (Maudsley Personality Inventory); E (Extraversion scale of MPI); R (R scale of Guilford STDCR); D+C (sum of D and C scales of STDCR); MAS (Taylor Manifest Anxiety Scale) and N (Neuroticism).

(4) *Tests of heterogeneity of variances are sometimes lacking where they might be appropriate.* Some of Hildebrand's tests (1953) yielded variances which were significantly different among the groups which were pooled for his factor analysis. It is not known whether the analysis was substantially affected by this; perhaps not. At any rate, Eysenck cites the Hildebrand thesis as support for his dimensional hypotheses.

(5) Eysenck's statement that individual differences in excitation and inhibition 'are properties of the physical structures involved in making stimulus-response connections' implies that measures of personality dimensions based on these differences should be relatively stable over time within the same individuals. *However, no longitudinal studies of stability or shift of his dimensions have been conducted.*

(6) *Factors are identified by the same name from one analysis to another without any demonstration of their comparability.* Methods exist for comparing factors in separate analyses (Ahmavaara, 1954). A desirable method is to correlate factor scores (Thomson, 1951) on factors independently identified in two separate analyses for subjects in a third sample who have been given all the tests defining the factors.

(7) *When an article is quoted or a figure reproduced, items which are not fully consistent with Eysenck's hypotheses may be altered or omitted.* For example, the Trouton & Maxwell study (1956) is cited as one of the experiments establishing neuroticism and psychoticism as independent dimensions. Two of the factors emerging from the analysis (there were six in all) are discussed and a figure is presented showing items neatly and snugly clustering about the neuroticism and psychoticism axes, with only one item, depression, falling between. A reader comparing this figure with the figure published in the Trouton & Maxwell article will find that a number of items, such as compulsive acts, lacks confidence in society, agitated, irritable, recovered or much improved at time of discharge, and others, are omitted in Eysenck's representation. They are not omitted

on the basis of having the lowest factor loadings. Here is part of Eysenck's conclusion regarding this study: 'More important, however, than this verification of the existence of these two factors is the demonstration that they are orthogonal, that is to say, situated at right angles and quite independent of each other.' The situation seen in the Trouton & Maxwell article is not so clear-cut; oblique factors appear as reasonable as orthogonal ones. Lorr & O'Connor (1957) have criticized the Trouton & Maxwell analysis. Using the same data, they rotated the frame of reference to an equally justifiable set of factors.

* * * *

Other tendencies worthy of note are not discussed here but will be mentioned when the most recent examples, those occurring in Eysenck's presentation of the evidence for his theory, are considered.

These general considerations are mentioned in order to caution the reader regarding the 'proofs' referred to by Eysenck. Some specific comments will now be made regarding the proof of the existence of the I-E (introversion-extroversion) dimension.

As part of his argument, Eysenck presents as 'a typical research' a study by Hildebrand (1953). In this study, a factor analysis was performed on the results obtained from the giving of 22 tests to six 'neurotic' groups and a 'normal' group. The hysteric and anxiety reaction groups were left out of the analysis to serve as criterion groups, and it was found that the factor scores on the factor identified as I-E calculated for these two groups discriminated between them significantly at the 5% level. The results were interpreted by Eysenck as being 'very much in line with the hypothesis' (1957, p. 30). Three other facts about the study should however be brought to the reader's attention: (a) Hildebrand nowhere states how he carried out his rotation of factor axes, only that he rotated them a certain amount. (b) The Guilford R scale had the highest loading on I-E after rotation. (c) The R scale was one of two scales to discriminate the two groups at the 1% level (*t* test). The finding that

a score most heavily weighted with a test already found to discriminate between hysterics and dysthymics also discriminates between the same two groups, though less well, might, in some circumstances, be considered trivial, especially since that test had been used as a basis for rotation. (Hildebrand has informed us, in a personal communication, that he rotated the I-E axis to the R scale).

It should also be pointed out that the Hildebrand tests were selected on rational grounds (supposed relevance to neuroticism or I-E) or because they had in previous research discriminated neurotics from normals or hysterics from dysthymics. Yet two-thirds of the scores (18 of 27) failed to replicate previous results (Eysenck, 1947) or were significant in a direction inconsistent with Eysenck's predictions.

Furthermore, Storms (1958) has shown that discriminant functions computed for the Hildebrand data separate the hysteric and anxiety groups better than do the factor scores and that the dimensions defined by the discriminant weights bear little resemblance to Hildebrand's I-E. From the same data, he also reports that the psychopath and hysteric groups, which are pooled by Eysenck to serve as prototype extraverted neurotics, are the most widely separated among the neurotic groups by tests selected to represent the neuroticism and I-E dimensions. This evidence makes such pooling appear unjustified.

Discussing I-E, Eysenck presents a table (1957, p. 28) of differences between introverts and extraverts 'which have been demonstrated experimentally'. The following comments refer to this table:

According to the table, extraverts have a higher I.Q./vocabulary ratio. Hildebrand, in the above-mentioned study, found no such relationship; neither did other investigations (S. B. G. Eysenck, 1955; Foulds, 1956).

The table states that persistence is lower for extraverts than for introverts, but Hildebrand found that psychopaths (one of the prototype extraverted groups) had the highest leg persistence scores, and that only hysterics were significantly lower than psychopaths.

Extraverts are reported to prefer orectic as opposed to cognitive humour. Hildebrand (1953) designed a study hoping to improve upon Eysenck's earlier investigation (1947) and found no differences.

While Rorschach M % and D had loadings on a supposed I-E factor in a study cited in the table (Eysenck, 1956*b*), neither measure discriminated rated introverts from rated extraverts in the very study from which these data were derived (McLeod, 1953). A scale derived from the R scale had only a 0.09 loading on the same I-E factor.

Introverts are alleged to condition faster than extraverts. That this was not established will be shown when the two experiments cited in the table are discussed later in this paper.

Although Nichols (1955) found larger kinesi-
thetic after-effects for hysterics than for dysthymics, the results did not hold up when he compared extraverted (R scale) hysterics with introverted dysthymics, despite the expectation that the differences would be magnified.

It is true that hysteric groups have been found to be more inert than normals or anxiety states in their reactivity in cockpit experiments (Davis, 1946, 1948), but anxiety states have not been shown to be overactive.

Hysterics and psychopaths are said to be extraverted and dysthymics introverted. In order to test this hypothesis, Sigal *et al.* (1958) gave the MPI, a questionnaire constructed by Eysenck (1956) and containing purified (by internal consistency methods) measures of I-E and neuroticism, to an hysteric-psychopath group and a group of dysthymics. The groups were selected with great care and manifested symptoms consistent with Eysenck's earlier descriptions (1947, 1953). The two groups were found to differ significantly from each other, but they differed on the measure of neuroticism, not I-E. (The hysteric-psychopath group was less neurotic.) The hysteric-psychopath group had a mean extraversion score almost identical with that given by Eysenck (1956*e*) for normals. When hysterics and psychopaths were considered separately, psychopaths were found to be more extraverted

than dysthymics, but hysterics were found to be on the introverted side of normals. Hysterics were significantly different from dysthymics but, once again, on neuroticism, not extraversion. This study was completed too late for inclusion in Eysenck's book, but in at least three other studies (Hildebrand, 1953; Martin, 1955; Nichols, 1955) hysterics had previously been found to lie on the introverted side of normals, though not significantly so, with respect to R scale scores.

It would seem, from this brief survey, that the attributes of extraverts and introverts listed in Eysenck's table 6 have not been unequivocally demonstrated. It also appears that the factor analytically based scales of I-E have never been successfully validated against an outside criterion. Those scales for which evidence is available fail to place hysterics toward the extraverted end of the I-E continuum, yet Eysenck accepts them as valid measures of I-E. One is forced to conclude either that the Typological Postulate is wrong or that a measure of I-E appropriate to the theory has not been found, or both.

The results of the studies just described have a considerable bearing on Eysenck's 'double screening' argument and, consequently, on the relevance to his theory of certain of the experiments which he adduces as evidence (e.g. Eysenck, 1955*a*; Franks, 1954). He argues (1957, pp. 31-3) that since hysterics are generally extraverts and the R or E scales have been shown to be good measures of I-E, a most satisfactory way of selecting a pure group of neurotic extraverts is to eliminate from a sample of hysterics any persons having low R or E scores. In the light of the preceding evidence, it is clear that studies conducted in this way may provide some information about extraverted hysterics but do not provide any information about hysterics in general. Also, until the effects of I-E and dysthymia-hysteria are separated, they provide little information relevant to Eysenck's postulates. By begging the question of the similarity of I-E to hysteria-dysthymia, such studies do not allow a test of this implication of the theory.

In his fourth chapter, Eysenck discusses Pavlovian notions, sets forth his own theory, and begins to present evidence relating to it. It may aid the reader of the book if we emphasize the fact that Eysenck interprets Pavlov in his own way and re-defines (though not very explicitly) the Pavlovian inhibition terms.

Having discussed the sections in which the groundwork is laid for the rest of the book, we now turn to a discussion of some of the experimental evidence presented in support of the theory.

THE EXPERIMENTAL EVIDENCE

Conditioning

After presenting his postulates, Eysenck proceeds to derive an hypothesis stating that people in whom excitatory potentials are generated rapidly and inhibitory potentials slowly and who are therefore introverted should form conditioned reflexes quickly and strongly. He then discusses what he terms 'a crucial experiment to test the predictive accuracy of our theory as compared to Spence's' (p. 115). The experiment consisted of giving a series of eye-blink conditioning and extinction trials to groups of 29 dysthymics, 20 'hysterics' (combined hysterics and psychopaths), 20 schizophrenics, and 20 normals. Several questionnaire tests were also given to all subjects. The dysthymic and hysteric groups were further selected on the basis of having introverted and extraverted questionnaire scores, respectively. The results of this experiment, performed by Franks (1956*a, b*, 1957), were that dysthymics produced significantly more eye-blinks in the 18 test trials during conditioning as compared with hysterics and normals. The latter two groups were not significantly different. (It may be noted that the graph presented by Eysenck contains certain errors which make the groups appear to differ more than was actually found by Franks.) The hysteric, dysthymic and normal groups were combined for the computation of a correlation of -0.48 between eye-blink and extraversion scores. Eysenck recognizes that this correlation is 'slightly inflated'

(p. 118), because the hysterics and dysthymics had been further selected as extreme scorers on the same measure of extraversion used in computing the correlation. Franks's schizophrenic group is not mentioned by Eysenck. Schizophrenics yielded the most introverted questionnaire scores but conditioned most poorly (not significantly different from normals). Perhaps complicating factors produced these discrepant results.

Eysenck discusses the results of this experiment in terms of 'conditionability' as though it had been demonstrated that conditionability is a general and unitary trait (p. 120). The available evidence suggests that different conditioning measures either do not correlate or are related very slightly (Razran, 1939; Roff, Payne & Moore, 1954), and little generality of individual differences among different measures of conditioning has been shown. It would seem therefore that Eysenck would have been well advised to speak of eye-blink acquisition rather than conditionability.

To make a case for the Franks experiment as a crucial comparison of his theory and Spence's hypothesis that eye-blink conditioning is influenced by anxiety operating as a drive, Eysenck argues that the MAS is loaded on extraversion. This loading, he believes, accounts for the relationships which have been found between the MAS and eye-blink conditioning. Correlations between the questionnaires used in the Franks study are given in Table 1. It will be seen that the MAS, the

R scale as a measure of I-E. It therefore seems quite possible that R is loaded on neuroticism as well as on I-E (in the Hildebrand analysis, the loading was -0.25) or that neuroticism and I-E are non-orthogonal. Furthermore, not only were the hysterics significantly lower than dysthymics on the MAS (which fact Eysenck attributes to the loading on I-E), but they were also significantly lower on D+C, the best measure of neuroticism in the Hildebrand analysis.

In the light of these facts, and the fact that there was no evidence that even extraverted hysterics gave fewer blinks than normals, it is difficult to see how the experiment can be regarded as a crucial comparison of the theories and the results considered to demonstrate the superiority of the one expounded by Eysenck.

Let us further consider the conditioning experiment apart from questionnaire results. Because the dysthymics emitted more eye-blinks following the CS in 18 test trials during conditioning, it was concluded that they conditioned more rapidly than hysterics. If the rates of conditioning for the two groups were different, the differences, in terms of number of blinks, in later test trials would be greater than in earlier ones. In other words, the learning curves would not be parallel but would diverge. An analysis of variance which does not involve assumptions about the forms of the learning curves was carried out by the authors in order to test this expectation. The first six, middle six, and last six test trials were used to provide scores with a reasonable range. If the curves were significantly non-parallel, the diagnosis by trial interaction would be statistically significant. It can be seen from Table 2 that this was not the case. The failure to obtain significance cannot be rationalized by contending that all learning had taken place within the first set of trials, since the comparison between sets of trials was significant.

It might still be claimed that a significant difference in number of blinks during conditioning is strong evidence that two groups equated for initial blink sensitivity to the tone learned the eye-blink reflex at differential rates.

Table 1. *Intercorrelations among questionnaire tests*

(From Franks's experiment (1954).)

Test	D+C	MAS	R
MMQ	0.880	0.923	-0.450
D+C	—	0.864	-0.370
MAS	—	—	-0.485

MMQ, and the Guilford D+C intercorrelate most strongly, and that all three also have significant correlations with the R scale. The MMQ and D+C scores have been used by Eysenck as measures of neuroticism and the

Table 2. *Analysis of variance of Franks's eye-blink acquisition scores (1954)*

Source	D.F.	Mean square variance	F	P
Diagnosis	1	149.6333	22.833	0.001
Trials	2	18.2583	15.288	0.001
Trials by Diag. Interaction	2	3.3583	2.812	0.05
Individual differences	38	6.5535	5.487	0.01
Residual	76	1.1943	—	—
Total	119	—	—	—

But were the groups in fact equated on sensitivity to the tone? One of Franks's selection criteria was that no one would be retained in the sample who blinked to any of three presentations of the tone alone before conditioning began; thus all subjects and therefore all groups had initial scores of zero blinks. However, these zero scores were arbitrarily forced to be zero. It may be that more persons of one group than of the other would blink at least once in another series of three tone presentations. It should also be pointed out that the tone alone trials previous to conditioning should have consisted of 18 presentations in order to provide comparability to the test series scores.

Another possibility is that at least some of the difference between hysterics and dysthymics in total number of blinks during the test series could be due to a higher spontaneous blink rate among dysthymics. The only evidence Franks provides on this point is a count of the number of spontaneous blinks of the subjects over a 1-minute period following the conditioning and extinction trials. During this period, dysthymics were found to produce significantly ($P < 0.05$) more spontaneous blinks than normals. Hysterics were not significantly different from normals and the difference between hysterics and dysthymics (more blinks for the latter) failed to reach significance ($P < 0.20$ on a two-tailed test). However, a 1-minute period may not yield a stable measure of blink rate, and greater differences might be found with longer time intervals; indeed the results can only be con-

sidered as suggestive. The possibility of differential spontaneous blink rate being a factor influencing so-called 'conditioning' scores cannot be rejected.

We have seen that the evidence that dysthymics condition more rapidly than hysterics is open to serious question. What about the groups postulated to be the normal counterparts of these neurotic groups? Eysenck refers to another study by Franks (1957), in which introverts and extraverts were chosen from a group of normal volunteers on the basis of the Guilford R scale. While criticisms regarding multiple selection criteria do not apply to this experiment, the preceding discussions about initial sensitivity to the CS and spontaneous blink rate are just as applicable here. Again, the analysis of variance not performed by Franks was done by the present authors. As Franks reports, there was a significant difference between introverts and extraverts in total number of blinks during conditioning. As expected, introverts produced more blinks. However, it was again found that the learning curves for the two groups were not significantly non-parallel, so that it cannot be said that learning progressed at different rates. Comparison with initial number of blinks to the tone alone is again impossible because of arbitrarily forced zero scores.

In both conditioning experiments, significant correlations between extraversion scores and total number of eye-blinks were reported. Apart from these correlations being inflated because the subjects used were chosen as extreme scorers on one of the measures involved,

the correlations can be most reasonably regarded as reflecting relationships between whatever the R scale measures and eye-blink reactivity to stimuli and/or spontaneous eye-blink rate. *There is no evidence that conditionability enters into these relationships.*

The studies of eye-blink conditioning using CNS excitant (dextedrine) and depressant (sodium amytal) drugs are no less equivocal. Eysenck himself offers one of the main reasons for questioning the relevance of the studies cited, viz. dextedrine is a sympathetomimetic drug. If his theory relating autonomic lability to neuroticism is correct, the drug may change the subjects' status on neuroticism as well as on I-E. Results of the experiments could, therefore, be due to differences on I-E or neuroticism, or both in unknown proportions. One of the studies cited by Eysenck (Lavery & Franks, 1956) provides another reason for questioning the findings. It was found that subjects receiving amytal showed significantly fewer bodily movements during the conditioning session than did controls. Thus, amytal may simply inhibit motor activity, eye-blink included. Eysenck, and Franks & Lavery (1955) rightly caution that the experiments may not provide evidence that the subjects receiving amytal did condition poorly. To test this, it is necessary to retest the same subjects for existence of the CR after the effects of the drugs have dissipated.

Sedation threshold

The sedation threshold refers to a stage at which certain objectively measurable reactions are supposed to take place when sodium amytal is administered intravenously at a fixed rate. In an earlier article, Shagass & Naiman (1956) reported that hysterics had lower sedation thresholds than obsessives. Eysenck attributed this finding to the expectation that less amytal would be required to produce an inhibitory effect indicative of marked extraversion in hysterics than in dysthymics, since the former are more extraverted and therefore already more inhibited or more prone to develop inhibition.

More recently, in a paper probably published too late for inclusion in *The Dynamics of Anxiety and Hysteria*, Shagass, Mihalik & Jones (1957) state that 'present data contradict the hypothesis that the sedation threshold may be a relatively enduring biological characteristic, perhaps determined on a constitutional basis'. They think that there is now evidence that lower thresholds are related to a state of symptomatic remission. This finding is consistent with the possibility mentioned earlier. Sodium amytal too, may alter the subject's position on neuroticism rather than (or as well as) on I-E, and, as a result, without the exercise of further controls, the relevance of studies using this or similar drugs is in doubt.

Inert versus overactive behaviour

A deduction for which Eysenck claims 'a considerable amount of experimental support' (p. 134) is given as follows: 'The strongly inhibitory type (extravert; hysteric) should on this hypothesis be subject to sluggish, inert response behaviour; the weakly inhibitory type (introvert; dysthymic) should be subject to overactive, over-responsive behaviour.' The work of Davis (1946, 1948, 1949) is cited as 'of particular relevance' to this deduction. Davis did not use measures of I-E, but he did include anxiety reaction and hysteric groups among his subjects. Eysenck presents some of the Davis data in his tables 9 and 10. The neurotics showed more abnormal (inert or overactive) responses and the hysterics produced more inert reactions. However, using the data in Eysenck's tables, one finds that neither anxiety reaction group is significantly more overactive than normals. It is true that in table 10, the χ^2 comparing normals and anxiety reactions is significant ($P < 0.05$), as is the related Whitfield's τ reported by Davis (1946), but this is due to a difference in variances ($F = 2.28$; $P < 0.05$) and not to a difference in means (Cochran & Cox test $CR = 1.08$; $P < 0.20$). When only the normals and anxiety states who showed abnormal reactions, as reported in Eysenck's table 9, are considered, a non-significant fourfold χ^2 is found ($P > 0.30$).

One wonders why Eysenck used only the anxiety states as dysthymics; perhaps because the 'other' neurotics were significantly more *inert* than normals in their responsiveness ($P < 0.01$).

A study by Venables (1955) related to this active-inert question is presented in considerable detail by Eysenck. Venables used two samples of subjects: 210 normal subjects and a neurotic group consisting of 11 hysterics (H) and 11 anxiety states (A) selected by the 'double criterion' method discussed earlier (Eysenck neglects to mention that the groups were further screened in this way). In order to compare the performance of the neurotic subgroups with that of theoretically equivalent normal subgroups, two groups of 11 subjects each were selected from the normal group on the basis of high scores on an I-E factor. These groups were called the QA (quasi anxious) and QH (quasi hysteric) groups because, psychometrically at least, they were similar to the A and H groups, respectively. The task the subjects were given involved a simple motor response to visually presented signals. In the first and last periods (A and C), signals were presented at 2-second intervals. In the intervening period (B), the task was made more stressful by presenting the stimuli at 1.5-second intervals. As a result of a factor analysis of various activity measures in this experiment, a score, R/W (size of response per unit time), was considered to be the best measure of activity.

Eysenck says that R/W was shown to be related to I-E but not to neuroticism or to intelligence. This he must have inferred from the fact that a significant ($P < 0.05$) correlation was found between rank on R/W and the anxious-hysteric dichotomy in the neurotic group during period A. However, the same correlation was not significant in periods B or C, nor was it significant in any period for the normal groups.

When the A and QA groups were compared for activity with the H and QH groups, respectively, only three of the 18 possible comparisons (three different measures of activity

were used) were significant. In no instance was a significant difference found between the QA and QH groups where it was found between the A and H groups, and vice versa. Furthermore, when one of the six possible R/W differences was found to be significant, the corresponding R/W difference for the theoretically equivalent groups was in the opposite direction, although not significant. Indeed, the signs were opposite for these supposedly corresponding pairs of differences in five of the nine comparisons made. It is also interesting to note that in period A the QH and A groups had almost identical R/W scores; in period C, this situation repeated itself, and in addition, the QA and H groups had almost identical R/W mean scores.

It can hardly be said that this part of Venables's study confirms Eysenck's 'Typological Postulate'. It is even less obvious why Eysenck maintains that the differences between R/W scores between periods A and B (R/W_{A-B}) and between B and C (R/W_{B-C}) for each group are in any way consistent with his theory. Eysenck says that 'emotional stress' leads to a 'disorganization of motor processes' and 'that the direction taken by this disorganization—inertia or overactivity—is related to extraversion-introversion, as postulated by our theory' (p. 139). There is nothing in Eysenck's postulates which says that increased emotional stress leads to increased inhibition potential for extraverts and decreased inhibition potential for dysthymics. Such a statement, which says, in effect, that high neuroticism accentuates I-E, would be necessary in order to interpret the significant correlation between R/W_{B-C} and I-E as supporting his theory.

There is one last point to be derived from Venables's data. If disorganization of response is related to neuroticism and the active-inert continuum related to I-E as Eysenck maintains (p. 139) and if the disorganization factor obtained by Venables reflects the type of disorganization referred to by Eysenck, then there should be no correlation (or a very slight one) between Venables's 'activity' and 'disorganization' factors. In actual fact, the

correlation was approximately 0.87. Variability measures constituted three of the four scores with substantial positive loadings on the disorganization factor. According to Eysenck's table 6, one would expect hysterics and extraverts to be more variable in performance than dysthymics and introverts; one might, therefore, expect a negative correlation between the activity and disorganization factors obtained by Venables. Although he does not make it entirely clear that disorganization accompanies overactivity, Venables reports the positive correlation given above. Thus, whether or not the disorganization factor measures what Eysenck terms disorganization, predictions which supposedly follow from the theory are contradicted.

Reminiscence

Since massed practice is supposed to generate more reactive inhibition and, hence, depress performance, and since extraverts and hysterics are supposed to generate inhibition rapidly, Eysenck predicts that they should show a more marked improvement in performance (greater reminiscence effect) following rest than introverts and dysthymics, provided the rest pause is long enough to permit most of the reactive inhibition to dissipate.

Using pursuit rotor performance of 50 normal subjects over three 5-minute periods separated by two 10-minute rest pauses, Eysenck found a correlation of 0.29 between the E scale of the MPI and the first reminiscence score (1956*f*). This correlation, significant at the 2% level, he reports in the book (p. 126); the insignificant correlation of 0.10 between the E scale and the second reminiscence score is not reported. He mentions that a significant correlation between neuroticism scores and reminiscence was found. Actually, neuroticism correlated 0.40 ($P < 0.01$) and 0.27 ($P < 0.05$) with the two reminiscence scores (Eysenck, 1956*f*). It is interesting in this context to consider Eysenck's statement about the Taylor and Spence correlations between anxiety and conditioning: 'The correlations observed under these conditions are in the range of 0.25...in

other words, only about 6% of the variance is common to the two variables. This relationship is so slight that it is difficult to regard it as very impressive, even if one were entitled to assume that the demonstration of a relation between conditioning and MAS did unequivocally support the Spence-Taylor theory' (p. 91). The correlation of 0.29 between I-E and reminiscence accounts for about 8% of the variance. That the correlation is not higher is rationalized by Eysenck as being due to the low reliability (0.44) of the reminiscence score. However, the neuroticism scale correlated 0.40 with reminiscence in one instance, almost as well as reminiscence with itself.

Eysenck quite rightly points out the merits of an 'independent duplication' of these results. However, there are aspects of the Treadwell study (1956), which he cites as such a duplication, which make it doubtful that it can be so considered. Using the T scale from the TSE (Evans & McConnell, 1941) to select her groups (the TSE is not derived from the MMPI as Eysenck states), Treadwell conducted an experiment with a tracking apparatus and used a procedure which permitted the computation of reminiscence scores. As Eysenck reports (pp. 126, 127), extreme T scale extraverts showed significantly more reminiscence than introverts. He concludes: 'Thus these results, obtained under quite different circumstances, agree with our own work in supporting the view that inhibition is stronger in extraverts than in introverts' (p. 127). The difficulty with this conclusion is that the T scale (thinking extraversion) had not been related to other measures of extraversion, so it could not be assumed that it measures extraversion in Eysenck's sense. The only data we could find relating the T scale to the MPI neuroticism and extraversion scores is contained in a recent thesis by Brebner (1957). Twenty-three university undergraduates were given both tests, and it was found that the correlations of T with the E and N scales of the MPI were 0.22 and -0.13 (our calculations; the 0.22 figure is probably inflated because only ex-

trème scorers on E were included in the sample). Neither correlation reaches the 5% level of significance. The lack of relationship between the tests suggests that the Treadwell results may be irrelevant to a discussion of Eysenck's theory.

A study comparing the effects of amytal, dexedrine and a placebo on pursuit rotor performance is relevant to the problem of reminiscence. Eysenck cites the study (Eysenck, Casey & Trouton, 1957) in support of his theory because the level of the work curves of one of the dexedrine groups is higher than the level for the amytal group. However, he does not provide data for reminiscence scores, which could easily have been obtained. Inspection of the curves he does provide (p. 242) indicates that the introvertizing drug (dexedrine) produced more reminiscence than the extravertizing drug (amytal). This part of the data is more directly relevant to the theory, though inconsistent with it, than the parts discussed by Eysenck. Yet he fails to report the result or to indicate whether it is statistically significant.

Satiation and figural after effects

Eysenck expounds the Kohler & Wallach (1944) concept of satiation as a form of inhibition. While he summarizes Duncan's (1956) 'formal points of similarity' and cites Wertheimer (1955) as having presented evidence that satiation effects correlate across sense modalities, he adduces little evidence that satiation correlates with other supposed effects of inhibition such as differential conditioning rates or reminiscence. The one example cited is the Livson & Krech (1955) finding of correlations of 0.35 and 0.32 between figural after effect and retroactive inhibition. Nowhere else, however, does Eysenck relate retroactive inhibition, a concept quite different from reactive inhibition, to his theory. His 'differential proof' that satiation is related to reminiscence consists of showing that: '(1) satiation correlates with extraversion; (2) reminiscence correlates with extraversion' (p. 153). Apart from the doubtful status of the compo-

nent propositions, this may strike the reader as an example of the fallacy of the undistributed middle.

Assuming a correspondence between satiation and inhibition, Eysenck deduces that hysterics and extraverts will show larger and more persistent effects than dysthymics and introverts on a number of satiation phenomena. One of these is the kinesthetic figural after-effect. The study first discussed by Eysenck has also been presented by him in two previous publications (1955*a*, *b*). He found an over-all significant difference between extraverted (R scale) hysterics and introverted dysthymics in magnitude of figural after-effect. However, for the separate periods of stimulation between pre- and post-judgements, only the 30-second interval produced significant results, and the differences between the average scores of the two groups did not increase for longer periods of stimulation as one might expect from his theory if neither group had approached its inhibition asymptote. Furthermore, even the differences that were obtained would have been reduced and possibly would have disappeared if the subjects diagnosed as hysterics or dysthymics but not included because of their test scores had been included. Eysenck (1955*a*, p. 103) has stated that among the seven excluded for this or other reasons, the hysterics had the sort of after-effect he would have expected from dysthymics, and vice versa.

Nichols's kinesthetic figural after-effect study (1955) is also cited as support for the theory. Nichols did obtain a significant difference between non test-screened hysterics and dysthymics in amount of after-effect. What Eysenck does not report is that Nichols found no relationship between R scale and the effect, and when test screening using R was instituted, the procedure employed by Eysenck, the difference disappeared and went insignificantly in the opposite direction. It is interesting that the correlation of the after-effect with the Guilford R scale was -0.25 ($P < 0.05$), while the correlation with D+C, which Eysenck considers a measure of neuroticism (though not a pure one) was $+0.35$. Thus, this evidence,

not mentioned in Eysenck's discussion of Nichols's data, suggests that this measure of after-effect, the only one in Nichols's study which differentiated hysterics from dysthymics, is more related to neuroticism than to I-E. This is consistent with findings by Franks (1954) Hildebrand (1953), and Sigal, Star & Franks (1958) that dysthymics obtained significantly higher scores than hysterics on certain measures of neuroticism.

Eysenck discusses the Klein and Krech kinesthetic figural after-effect study in which brain-damaged subjects were considered to yield greater after-effects than normals, as Eysenck would expect, since he considers brain damage to increase one's susceptibility to inhibition. He also mentions the 'low statistical criterion' used by Klein and Krech. They found differences significant at the 10 % level using a one-tailed test. The only difference significant at the 5 % level was that after the first recovery period when differences should have been minimized. Eysenck mentions the Jaffe (1954) replication of this experiment. With improved sampling and a better controlled design, differences between Jaffe's 20 brain injured and 20 controls in no case approached significance ($P < 0.50$ in all instances). One can agree with Eysenck that 'it is doubtful whether we can legitimately use the results quoted (from the Klein and Krech experiment) as support of our hypothesis' (p. 160). It is interesting, however, to investigate Eysenck's footnoted claim (p. 160) that Jaffe (1955) later redeemed himself somewhat. Redemption consists of several case descriptions with no normal controls. The latter article was not intended by Jaffe to be a report of an experiment.

Among his experiments, Nichols included one which investigated a visual figural after-effect involving the comparison of circles. The effect was significant in both groups, but the difference between hysterics and dysthymics was insignificantly ($0.05 < P < 0.10$) in the opposite direction from the expectation on the basis of the theory (hysterics should show more of the effect). The correlation of comparison of circles with kinesthetic figural after-

effect was 0.15 (not significant), whereas the latter correlated -0.30 with intelligence ($P < 0.05$). This constitutes no evidence for a unitary process underlying both after-effect measures.

Eysenck dismisses the sections of Nichols's data and questions the results of others dealing with '... visual satiation, speed of reversal of ambiguous figures, after effects of rotating spirals, and dark vision...' (p. 154) as being '... vitiated by failure to control...' visual fixation. All Nichols said on this subject is: 'The hypothesis advanced on a number of occasions, regarding the ability to fixate, may have accounted for the discrepancies with the visual tests.' This statement was made as one of a series of attempts to account for the discrepant findings without considering them to invalidate the Eysenck theory, and no evidence was presented to support this as a valid objection to the acceptance of his (Nichols's) findings. Although Eysenck also provides no experimental evidence that hysterics fixated either less well or better than dysthymics (this is, after all, the appropriate evidence to be sought in accepting or rejecting Nichols's data), he does say that Granger (1957) found no such differences in a study of dark adaptation.

Nichols's subjects were tested before and after supposedly satiation-producing stimulation in studies of apparent motion threshold, auditory localization, auditory discrimination, and horizontal adjustment of lines. In no case were the results significantly in the predicted direction in terms of magnitude of after-effect. The auditory discrimination test produced results almost significantly in favour of the theory ($P < 0.10$, one-tailed test), but the adjustment of lines test produced results almost significant in the opposite direction ($P < 0.10$, two-tailed test). The uncorrected reliabilities (immediate test-retest) were all between 0.83 and 0.94, but the highest of the intercorrelations of the six measures was 0.20 (not significant). The separate results provide little support for Eysenck's theory, and the interrelationships constitute no evidence for the existence of a satiation or an inhibition factor.

Eysenck describes experiments (pp. 163-6) on the Archimedes spiral after-effect (subjective reversal of the spiral after it ceases to rotate). While hysterics and extraverts are expected to have longer and stronger after-effects than dysthymics and introverts in other kinds of experiments, due to inhibition set up by the interpolated stimulation, the spiral after-effect is predicted to be shorter for the former groups. The after-effect is considered to be the result of some photochemical or neurophysiological process which leads to neural activity. The neural activity, in turn, produces inhibition, so the faster inhibition is generated, the shorter the after-effect. This latter explanation fails to take into account the inhibition which presumably would be set up by the original viewing of the rotating spiral itself. It is not clear how Eysenck's prediction follows from the theory or how the additional assumptions made in the attempted derivation are justified.

It is also noteworthy that Wertheimer, in a paper (1955) cited by Eysenck in another connexion, has reported that demerol, seconal, and alcohol, all of which are depressant and therefore supposedly extravertizing drugs, led to decreased figural after-effects, a finding opposite to the appropriate prediction from Eysenck's theory.

Dark adaptation

Eysenck devotes a number of pages (pp. 166-70) to a study by Granger (1957) in which the dark adaptation curves of hysterics, dysthymics, normal extraverts, and normal introverts were obtained. This is one of the few cases in which the same experiment was performed using hysterics and dysthymics, not further screened on the basis of questionnaire scores, as well as their normal counterparts defined according to test scores. However, it is another instance of a conclusion which does not seem to follow from the data.

Throughout the course of the adaptation curves, hysterics had higher thresholds than dysthymics, and the difference is significant, as Eysenck would predict. For the normal

introverts and extraverts, however, all differences were decidedly non-significant, and in the initial portions of the curves, the trend was in a direction contrary to expectation.

The curves for hysterics and dysthymics should have converged as the satiation effects (which Eysenck hypothesized as the cause of the higher threshold in hysterics) dissipated. For the entire experiment, however, they remain remarkably parallel. In an attempt to account for this, Eysenck speaks of two kinds of inhibitory potential which interact in the later portions of the adaptation process to produce the effect. The concepts involved are not derived from Hull nor from Eysenck's postulates, which involve only reactive inhibition, supposedly generated more quickly and dissipated more slowly in hysterics than in dysthymics. Here is another instance of ambiguity in the 'deductions' derived from the theory and a case where additional assumptions are made.

It is not clear why Eysenck concludes that '...the results are in agreement with prediction...'. Granger's conclusions would seem to be more in accord with the experimental results. He failed to find evidence that his differences reflect differences in cortical inhibition. Nor did he find '...any satisfactory evidence that these measurements are significantly related to relatively permanent personality differences between individuals in the way demanded by Eysenck's theory, even though thresholds may be affected by psychiatric disorder' (1957, p. 119).

Apparent movement

Eysenck predicts that '...hysterics, extraverts, and people with brain damage would have higher thresholds' (p. 171) for the perception of apparent movement. Since such people are more prone to develop inhibition or satiation, the spread of excitation from one part of the cortex to another should be dampened, with the result that more rapid alternation of stimuli (i.e. shorter time intervals between presentations) would be required to produce the subjective perception of movement.

As confirmation for the theory, the Werner & Thuma study (1942*a*) is reported. In that investigation, only two of 20 brain-damaged subjects but 19 of 20 mental defectives (not normals as stated by Eysenck) saw movement when two black cues displaced angularly from each other were exposed successively in a modified Dodge tachistoscope. It should be pointed out, however, that in the same study, 15 of the 20 brain-damaged subjects did report movement when presented with a meaningful clock figure. The thresholds of these 15 tended, if anything, to be lower than those of the controls.

As Eysenck states, Brenner (1953*b*) found '...that only two of 23 brain-damaged subjects saw apparent movement in the same way as the control subjects', using the same modes of presentation as did Werner and Thuma. Eysenck does not report her deeper investigation of the matter, as a result of which she judged that the difference was due to a dependence on attributing meaning to the figures and that the brain-damaged subjects '...resemble mental defectives in that the time interval plays a secondary role in determining the report' (1953*b*, p. 95).

A study of the responses of normal subjects used in the same investigation suggested to Brenner that the reports of movement were not so much occasioned by actual perceptions of movement as by inferences made from the relative positions of the two successive stimuli. Thresholds obtained in this way were quite susceptible to such influences as suggestibility and set, so the differences observed might be due to these factors rather than 'cortical inhibition'.

To overcome these difficulties of the single presentation method, Brenner modified her procedure, presenting the stimuli cyclically (i.e. with the same time interval between *a* and *b* as that between *b* and *a*). She was able to obtain a clear perception of movement in all her normal subjects (whereas seven of 33 failed to report seeing movement with the procedure first used), and the brain-damaged subjects reported movement in simple as well

as complex figures. The threshold of change from apparent movement to apparent simultaneity of the two stimuli was considered to be a better measure of physiological effect than the threshold of change from succession to movement. Both thresholds should be influenced in the same direction by inhibition as conceived in Eysenck's theory. The poorer measure yielded a significant difference between groups, brain-damaged subjects perceiving movement at slower presentation speeds (longer time intervals) than normals. This result is in the direction opposite to that predicted by Eysenck. Results with the other threshold measure were not significant.

Nichols (1955) obtained a significant difference between hysterics and dysthymics on thresholds for apparent movement. The difference was in the predicted direction, but the correlation between threshold scores and extraversion (R scale scores) was not significant.

Another experiment by Brenner (1953*a*) is cited as a demonstration that the effects of satiation are the same as those of brain damage, confirming a deduction from the theory. The results of this study of the effects of continuous stimulation in any one of four modalities would be better support for the theory if comparable thresholds changed in the same direction as a result of satiation and of brain damage. Satiation produced a significant increase in the rate of presentation required for the perception of movement rather than succession, while the same threshold was significant in the opposite direction for brain-damaged subjects as compared with normals (Brenner, 1953*b*, 1956).

Rotation effects

Eysenck presents at some length Shapiro's (1952) explanation of the fact that brain-damaged subjects rotate their reproductions of block designs more than do normals. Substituting 'satiation' for 'negative induction', the term used by Shapiro, he predicts that the drawings of hysterics and extraverts should also be rotated more than those of dysthymics and introverts. Nichols's previously cited work (1955) provides the only available data

relevant to these predictions. The rotation test used by Nichols consisted of 24 of the 48 items used by Yates (1956). A non-significant difference (though in the predicted direction) was found between hysterics and dysthymics, while the correlation between rotation and the usual I-E measure (R scale) was -0.004 .

CFF

Since fusion of a flickering light into an apparently steady one above certain flicker frequencies is presumably due to spread of excitation, individuals who build up excitation more slowly or inhibition more rapidly should, on Eysenck's hypothesis, require more rapid flicker frequencies to produce subjective fusion. Thus, hysterical, extraverted, brain damaged, and amytal drug groups should have higher thresholds than dysthymic, introverted, non-brain damaged, and dexedrine groups. Eysenck cites several studies relating states of anxiety to lower thresholds than those obtained from normal groups, which accords with expectation as far as it goes. However, he fails to report studies presenting evidence that extravertizing drugs, brain damage, and possibly extraversion (other measures of extraversion than Eysenck's were used, so the results may be irrelevant) are also related to lower thresholds, which is contrary to his prediction (Battersby, 1951; Simonson & Brozek, 1952; Teuber & Bender, 1948; Werna & Thuma, 1942b).

Fortunately, Eysenck cautions that his attempt to relate CFF to I-E by way of its relationship to Alpha rhythm stands on tenuous grounds. While he does report the statistically significant results obtained in one (Chyatte, 1954) of the two studies of the relationship of CFF to Alpha index which he cites, he does not report that in the other study (Friedl, 1954),

a significant correlation was not obtained. It might be added that the results of the first study failed to replicate (Donders, Hofstaetter & O'Connor, 1956).

SUMMARY AND CONCLUSIONS

Comparing the preceding discussion with Eysenck's book, the reader can evaluate the degree of empirical support for the theory he propounds. To the authors, it appears that this intriguing theory is no more powerful in accounting for known facts relevant to the study of personality than are the theories of the psychoanalytic systems, although it may have the advantage of generating more experimentally testable predictions. While the derivations are not always unambiguous and logically rigorous, in the hands of its originator the theory has provided a productive source of hypotheses and a stimulus to research.

There is little doubt that the '...facts relating to satiation, conditioning, reminiscence, spiral after-effects, brain damage and so forth...' cannot be deduced from any psychoanalytic theory now in existence. *There are, however, grounds for doubting that these facts can be deduced from the theory advanced by Eysenck.* Since a great deal of experimental evidence, even in the studies he cites in support of the theory, cannot be explained within its framework, it is apparent that a theory fitting the data has yet to be written. No evidence has been discussed which raises questions about the principles of learning used as part of the theory, but it is clear that at least Eysenck's Typological Postulate and probably the one about individual differences require considerable modification to cover the available evidence. This necessity is not suggested by the book, which presents highly selected aspects of the relevant research findings.

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(Professor Eysenck is preparing a reply to this paper.)

A NOTE ON MOTHER-CHILD SEPARATION AS A MENTAL HEALTH HAZARD*

By JOHN BOWLBY

My attention has been drawn to the fact that various professional workers are implying that I no longer regard the separation of young children from their mother-figures as a serious mental-health hazard. Since this is not so, I am writing this note to describe my position.

My belief is that a misunderstanding has arisen from a paper published in this *Journal* eighteen months ago (Bowlby, Ainsworth, Boston & Rosenbluth, 1956). In it we presented the findings of a study in which data derived from teachers' reports and test performances of schoolchildren who had spent long periods in a sanatorium, beginning before their fourth birthdays, were compared with similar data on their classmates. Significant differences in the expected directions were found, but it was also true that 'more of the sanatorium children than had been expected were reported as able to make friends, and fewer appeared to show the severe disturbances in object relations which underlie persistent delinquency'. We concluded that the principal value of the investigation had been 'to display the great heterogeneity of personality organization which is consistent with having undergone a prolonged separation experience starting before the fourth birthday' and, therefore, that 'statements implying that children who are brought up in institutions or who suffer other forms of serious privation and deprivation in early life commonly develop psychopathic or affectionless characters (e.g. Bowlby, 1944) are seen to be mistaken'.

Although I believe that in including myself amongst those guilty of overstatement I may have been unduly self-critical, this is not the point of this note. What is of consequence here

* The substance of this note appeared as a letter to the editor of the *Lancet*, 1 March 1958. Manuscript received 29 August 1958.

is that, although the study showed that there was much variation of outcome, it did nothing to cast doubt on the many studies which indicate that *some* children in their personality development suffer grave damage and others lesser damage from a separation experience; nor did it detract in any way from the studies, such as that of Prugh, Staub, Sands, Kirschbaum & Lenihan (1953), which confirm the common observation that during a separation experience and after return home a majority of young children are emotionally disturbed. Summing up the lessons of our follow-up study we wrote: 'there are no grounds for complacency. . . . The disturbances (found) are serious and affect a far from negligible proportion of children. In so far as measures can be taken to prevent them it remains urgent they be taken.'

The position as I see it to-day is rather like what I suspect it may have been a generation or more ago in regard to poliomyelitis. Early workers, impressed by the severity of the aftermath in certain cases of the disease, may well have overestimated the proportion of patients who suffered residual paralysis. The fact that we now know that it is only a small minority who are left afflicted has not altered our estimate of it as a serious illness, to be prevented at all costs.

There are also differences from the case of poliomyelitis, however. In that disease we not only have some idea of the incidence of residual disability but have ready and reliable methods for estimating its degree. In the case of personality disturbance following separation and similar experiences we have neither. Lacking a basis for calculation, therefore, we are in no position to take calculated risks.

Were I now to prepare a revised edition of *Maternal Care and Mental Health* (Bowlby,

1951), it would of course be necessary to rewrite Part 1 so as to include many new studies, mostly confirmatory but some not so, and to take account of various criticisms which have appeared. Though the picture would appear more complex and the emphasis vary here and there, the over-all pattern I believe would look much the same. What is more important, the practical recommendations would stand. In my judgement the separation

of a young child from his mother-figure is not to be undertaken without weighty reasons, and then only provided there is a suitable and stable substitute available to care for him.

In conclusion, I wish to emphasize that the object of this note is not to persuade sceptics of the correctness of these views but to discourage anyone from supposing that I have changed my position in any material way.

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STUDYING THE PSYCHOANALYTIC PROCESS BY THE METHOD OF SHORT-RANGE PREDICTION AND JUDGEMENT*

By LEOPOLD BELLAK

Among theoreticians, there are those ambitious ones who like to be far removed and who study problems with a telescope; there are others who consider as gross any approach but the one by ultramicroscope. There are, of course, those who consider reality altogether too coarse and prefer to build their own models of the cosmos they care to study. I should have to label myself a *magnifying glass theoretician*: interested in problems just one step removed from what one can see with the naked clinical eye—indeed, a low-level theoretician.

To define my frame of reference further, it is necessary to enter some personal amendments to Morgan's canon of parsimony—never discard a theory until you have thoroughly exhausted all its possibilities. In my case this means a commitment to Freudian psychoanalytic theory which I believe still supplies the internally most consistent, most comprehensive theory of personality. Its hypotheses are poorly defined, poorly integrated, but are probably largely verifiable and useful in their ability to help understand, predict, and control behaviour, therapeutically and otherwise.

We know that research in psychotherapy has had to address itself to at least five major variables; the patient, the situational factor, the time factor, the therapist, and the therapeutic method. Most basically, we want to keep four of them constant and to examine only the effects of varying the therapeutic operations. The classical difficulties have of course been those of controls; patients, and situational factors effecting patient and therapist cannot be matched, nor can therapists be equated. The passage of time always brings up

the possibility of spontaneous changes, particularly if therapy takes years.

The therapeutic methods are, of course, by no means well enough standardized to be matched or compared. But even greater is the difficulty of defining criteria of change, or more specifically, criteria of beneficial therapeutic change. There are at least two dozen reports in the literature on the use of different criteria. The principal problems involved there are those of validity of the criterion, the proper unit of observation, and the problem of meaningful quantification. Last, but certainly not least, is always the problem of the distortion of the process by observation and the meaninglessness of unreal situations.

Before proceeding to a brief account of my own way of attempting to deal with these difficulties, I should seriously like to offer another addendum to Morgan's canon, something one might call a 'rule of experimental parsimony'—'never introduce more distorting factors, nor simplify an experimental situation, more than is absolutely necessary'. One needs an optimal balance between experimental cleanliness and realistic meaningfulness. To be methodologically orderly is not a virtue if it is at the expense of meaningfulness; and clinical procedure is useless if it cannot be tested at all. By virtue of dealing with approximations, all scientific procedure is a question of bargaining and the best scientist is the one who can strike the best bargain in approximating reality in terms of laws. Generally, obsessive people are too much in the market for cleanliness of procedure and feel disdain for dirty reality. Hysterical characters are likely to be carried away by their own phantasies of clinical omnipotence, or by perceptual regression (into lack of acuity). It is by oscillation in one person or among many people, between excess of per-

* Presented in the forum symposium on 'Theoretical and Methodological Problems of Research in Psychotherapy, A.P.A. meeting, New York, 1957. Manuscript received 1 May 1958.

ceptual acuity (to the point of isolation) and lack of it, in the service of the ego's function of creativity, that scientific progress is achieved.

As my concrete example of research I should like to refer briefly to an exploration of the psychoanalytic process conducted by myself, with Dr W. Brewster Smith and with the experimental collaboration of Drs Andre Allen, David Epstein, Bertram Gosliner, David Kairys, and the late Adolph Zeckel. A preliminary report has been previously published (Bellak & Smith, 1956).

The approach used might be called a 'method of repeated short-range prediction and judgement', and our general purpose in the study was to explore the psychotherapeutic process and the theoretical basis of the communications between psychoanalyst and patient so that what is implicit in practice could be made explicit and grounded in theory. More specifically, we set out to investigate the success *with which understanding, prediction, and the therapeutic control of behaviour was achieved through application* of psychoanalytic hypotheses, by interaction between patient and analyst.

Our study consisted of recording psychoanalytic sessions, and asking two psychoanalysts what had occurred in recorded psychoanalytic hours in a number of specific respects. Two other analysts were asked to attempt to predict independently what the analytic nature of subsequent sessions would be. Statistical comparisons of agreement and disagreement were made.

By this procedure (and with the use of electrical recording) psychoanalysis was made to fulfil a basic tenet of science: It became a 'publically demonstrable and repeatable experiment'. There is no reason why another group of analysts might not study the record independently to see if they too arrive at similar results.

We will have to forego here a discussion of most details and of the premises involved which can be found in the earlier report and which concern the concept of the psychoanalytic situation as an experimental setting, the concept of prediction, the matter of record-

ing sessions and the problem of quantification of personality variables.

Except for unobtrusive recording (with the patient's permission) no other artifacts entered to make these sessions different from any other analytic sessions. After a brief trial run was made on other material, two patients were recorded successively, each for about fifty sessions. The recorded material was typed, with identifying data deleted and was then distributed to all participants. Comments of the analyst, supplementing the auditory record (e.g. shifting position and voice of patient) were added to the transcript. All participants were allowed to listen to all or any part of the recordings if they wished, though in fact, predictions and judgements were based almost entirely on typewritten transcripts. Predictions and judgements were made with the help of forms provided for the purpose.

Two members of the group made predictions on the basis of the material. Starting one week later than the 'predictors', two other members of the group judged what had in fact happened in the analytic sessions, using basically the same forms as the predictors. The treating analyst also made predictions which were not, however, communicated to the participants.

After 4 weeks, a group meeting was held. The statistical findings on the first 4 weeks of comparisons of predictions and judgements were presented to the group. If necessary, modifications of method (such as adding or omitting variables) were adopted as the result of general discussion.

The arrangement was then changed: those who were 'judges' now served as 'predictors' and vice versa. After 4 weeks a group session again took place followed by another four week period of switched functions, and so on, to a total of 24 weeks. Different analysts were paired as predictors or judges in these periods, systematically varying the possible combinations. After the end of this project, a general evaluation took place with particular emphasis on the nature of hypotheses confirmed and those which were not. An attempt was made to

differentiate artifacts from genuine shortcomings of hypotheses and formulations.

The same procedure was followed for the second patient (though additional prediction-and-judgement sheets concerning multiple choice methods of identifying the main theme of sessions were introduced). On a quantitative prediction sheet the predictors were asked to rate a number of variables, such as transference, acting out, etc., for the present hour, the next hour, the hours of the next week, etc. They were asked to express their statements in terms of numbers—e.g. positive transference (8) meaning a good deal of positive transference, while positive transference (2) would mean rather little positive transference, etc.

An almost identical sheet existed for the judges. It was easier for them in that they had only to judge quantitatively, rather than to predict the future as well!

To increase the meaningfulness of the quantitative abstractions, all statements were tied meaningfully to qualitative statements made on the following page. On this second or qualitative page, predictors were asked to give their reasons for anticipating change in each variable so rated. They were asked to name the variable with respect to which they had predicted a change; to summarise the observed facts relevant to prediction of change; to state the hypothesis underlying their predictions, and thereupon to state the predicted changes of that particular variable. For the third page, in essence the same procedure was followed for *postdiction*—a term (of G. W. Allport) applied to statements of what must have happened in the past of the patient to account for presently observed clinical facts, that is, by prediction in reverse. Finally, on the fourth page, predictors were asked to write a thumbnail sketch about developments in the week's analysis, unhampered by quantitative ratings and syllogistic logic. Similar sets of four sheets were used for the judge.*

The results of the study are recorded in

* Copies of the recording sheets used are obtainable on request from Dr L. Bellak, 1160 Fifth Avenue, New York 29, New York, U.S.A.

detail in the publication referred to earlier. The degree of agreement among judges, among predictors, and between predictors and judges was assessed by correlation coefficients. All the correlations were positive, most of them moderately to strongly so. It has to be pointed out, however, that ratings were made only on those variables which were rated by all participants (they were not forced to rate all the variables all the time). Also, only those ratings on which there was high agreement between the judges themselves, were correlated with those of the predictors (otherwise there would have been no facts to test the predictions against).

These positive findings do not necessarily mean that the predictors were able to predict dynamic change correctly. While there is some basis for that inference, there is some that indicates that they usually agreed primarily on the persisting structure of the patient—that is, they predicted the absolute status of variables rather than positive or negative change quantitatively.

Two principal ways are open to improve the means of studying and predicting actual change by the method described; by making longer range studies, over at least a year or two (longer range predictions would permit very small daily variations to be cancelled out). Or to make shorter range predictions, as was suggested (in a personal communication) by R. A. Spitz. Predictions would be made on intrasession material; that is, the immediate response of the patient to a statement by the analyst might be predicted by the independent observers and checked against the already available transcript.

There is no doubt that increasingly better definition of analytic concepts, longer experimental training, and better acquaintance with the problems of quantitative rating on the part of analytic participants would be beneficial.

Altogether, the qualifying statements made should not obscure the fact that the results of the study show, at the very least, a gratifying measure of agreement in regard to how four or five analysts describe in psychoanalytic language the psychodynamics of a patient. This alone is

probably more than has been established experimentally and statistically before, as far as we know.

The qualitative data supplement and highlight many of the quantitative findings, particularly with regard to the meaning of concepts, and the need for more careful definitions. They suggest that many quantitative disagreements were more apparent than real. In any one week there might be a divergence in judgements and predictions, but subsequent weeks often showed that what one analyst saw first was seen sometime later by the second analyst and vice versa—so that, in effect, their agreement on the basic facts was greater when seen from an overall viewpoint than when segmented week by week.

CONCLUSIONS

The method of short-range prediction and judgement described appears to have the following advantage for dealing with some basic problems of research in psychotherapy (and related situations):

(1) The problem of control groups of patients with the difficulty of matching samples is avoided.

(2) The equally difficult problem of defining success in therapy is obviated.

(3) The time factor which bedevils all appraisal of therapy is irrelevant. Ordinarily in appraising the outcome of a two year psychotherapy one has to wonder how much a patient might have changed spontaneously. Here we deal with the process of interaction itself.

(4) The method enables one to use one case as a whole population for statistical purposes. We can deal with many units of behaviour simultaneously, and each can be predicted and judged independently.

(5) Conceptualizations by different therapists can be studied and compared for the 'personal equation'.

(6) Concrete behavioural samples are the starting point for explicit hypotheses which can be tested over varying periods of time and rejected, modified, or considered validated.

(7) Analysts can be expert judges—in distinction to those studies wherein analysts felt that insufficiently qualified observers misunderstood analytic problems.

(8) In distinction to atomistic signs of progress or change, whole units of behaviour are examined (not only change in breathing, semantic variations, etc.).

(9) The method is flexible. If a change of variable is necessary or some other variation is indicated, it can be introduced without disrupting the 'grand design'. Arbitrary small units such as four weeks of sessions can be treated independently statistically as complete experiments within an experiment.

(10) The method may be useful not only for the exploration of other forms of psychotherapy and for social work, but for all processes of interaction. Behaviour of small groups, emergence of leadership, behaviour on a sociological and anthropological level may be usefully studied.

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CLINICAL PROBLEMS AND EXPERIMENTAL RESEARCHES: A REPLY

By H. J. EYSENCK*

In his address from the chair to the medical section of the British Psychological Society, Dr Russell Davis (1958) took as his jumping off point my recent monograph: *The Dynamics of Anxiety and Hysteria* (Eysenck, 1957). As the conclusions which he draws are rather different from mine, it would seem worthwhile to examine very briefly the reasons for this disagreement. If these reasons are of a factual nature, it may be possible to reconcile our differences; if they are not, it may be possible to get a little clearer the precise underlying assumptions from which our respective conclusions derive.

Davis points out correctly that what I am trying to do is to derive the treatment of neurotic disorders from a general and consistent psychological theory based on laboratory investigations. His main objection to this appears to be that: 'Although the problems to be resolved are clinical, the hypotheses, the ideas, which govern the researches are formed in the laboratory. In consequence, the laboratory theory decides to which symptoms attention is devoted, and these symptoms are in general not those to which clinicians attach major importance.'

There are two points to be made in reply. In the first place the objection was anticipated and answered on pp. 271 *et seq.* of my book, where I say: 'It might be argued that the papers referred to deal with monosymptomatic disorders quite unlike the usual run of anxious and depressed patients who seek help from the psychiatrist. How would a theory of the type here presented fare when applied to patients of this kind?' I then go on to quote in detail the work of Wolpe (1954) which deals precisely with the types of symptoms 'to which clinicians attach major importance'. Wolpe has demonstrated in the papers I quote, and even more so

in his recent book (Wolpe, 1958), that learning theory does in fact generate hypotheses relating to treatment which apply to the major neurotic disorders and permit deductions regarding treatment to be made with considerable success. Davis's statement, therefore, is factually incorrect. Theories originating in the laboratory have in a large number of cases been applied with considerable success to patients suffering from the classical neurotic disorders more usually treated by psychotherapy.

My second point is this. We have in some of our work dealt with disorders such as tics, enuresis, writers' cramp, etc. for the very simple reason that the symptom can easily be quantified and thus treated as a dependent variable in an experimental design in which the treatment constitutes the independent variable. Our interest in these cases was not so much in curing the patient (which was incidental to the experiment), but in showing that the symptom responded in a very precise manner which could be predicted in terms of our theory, to variations in the treatment. In other words, we were interested in showing that psychoneurotic symptoms can be dealt with in the accepted experimental fashion and as laboratory phenomena; it was only natural that our choice was governed largely by practical considerations of this kind.

But surely the antithesis which Davis is making between clinical problems and laboratory hypotheses is an entirely artificial one. A physicist may be interested in the causes of lightning, the movement of the heavenly bodies or the meandering of rivers; these correspond to the 'clinical problems' of the psychiatrist. History has shown that success in understanding and controlling these phenomena does not usually come from their direct study, but rather from laboratory investigations into the nature of electricity, the rate of

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falling of different bodies or the behaviour of small-scale models in the laboratory. It is only when a reasonable theory has been elaborated on the basis of such laboratory experiments, a theory which can then be extended to the natural phenomena under consideration, that we can begin to obtain a proper understanding of these phenomena. And in checking the accuracy of his hypotheses, the physicist, no less than the psychologist, will in the first instance test those deductions which can most easily be quantified and measured. This is not to say that other phenomena must not also be dealt with in the same manner; I am merely pointing out what has always been the strategy of scientists in coming to grips with a complex and difficult subject.

Davis takes me to task for not having proved that the methods I advocate are generally more effective than are other methods. This again is not quite true. To go back again to Wolpe, whom I quote on this point: he has shown on the basis of a statistical comparison that methods deduced from learning theory are significantly more efficacious in the treatment of the major neurotic disorders than are psychoanalytic methods. It may be possible to take issue with him on various grounds such as selection of subjects, criteria of improvement, etc., but this is not what Davis does in fact do. He denies the very existence of such data, and here again, therefore, we must conclude that his discussion is based on a factual error.

Having dealt with errors of fact, we now come to a difference in assumptions. He states that 'It is generally assumed that psychotherapy based broadly on the suppositions enunciated by Breuer and Freud is effective in a wide range of cases... the assumption has been little if at all weakened by Eysenck's (1952) much discussed failure to demonstrate statistically in large pooled samples that "psychotherapy facilitates recovery from neurotic disorder"'. First of all, we may ask ourselves how widely held this assumption in fact is. Dr Weinstock, chairman of the Fact Finding Committee of the American Psychoanalytic Association, stated categorically, in a lecture delivered at the

Maudsley Hospital, that *the American Psychoanalytic Association does not make any claims regarding the therapeutic effectiveness of psychoanalytic methods*. Similarly, Glover (1955), in his recent book, has explicitly disowned the assumption of therapeutic effectiveness of psychoanalytic methods. Many former psychoanalysts, such as for instance Albert Ellis, have expressed their lack of faith in precisely the assumption made by Davis, and have tried to elaborate new and better methods to supersede the psychoanalytic mode. If we must work with assumptions, then it would be interesting to know precisely who is making the assumption, and why such expert bodies and people as those mentioned are not willing to make it. Davis in fact is making his task a little too easy by simply disregarding the evidence and pointing to a certain assumed temporal contiguity of treatment and cure. The notion that *post hoc ergo propter hoc* is a valid logical principle has been hard a-dying, and apparently is still not quite defunct.*

* Altogether the logic of the argument somehow escapes me. Davis is concerned with abreaction and the recall of traumatic experiences, but abreactions in many ways similar to those observed by Breuer and Freud were described and deliberately produced a hundred years earlier by Mesmer, who also claimed considerable successes for his methods. He also used the alleged successes as an argument in favour of his theory of celestial magnetism. It would be easy to rephrase Davis's argument to read: 'It is generally assumed that magnetic therapy based broadly on the suppositions enunciated by Mesmer is effective in a wide range of cases...' Would Davis go on to say that the experimental psychologist should take his cue from such a 'clinical' statement and concentrate his energies on the study of magnetism? The assumptions made by Mesmer and his followers were based on the same kinds of observation as those made by Freud and his followers, and both parties adduced many examples of therapeutic successes allegedly due to their methods of treatment based on their particular theories. Why should we accept one and reject the other argument in the absence of those experimental and statistical studies which alone can give us a rational cause for effecting such a choice?

My own position, of course, is a very simple one, namely that in such an important area assumptions, even if they were as widely held as Davis mistakenly believes, are not sufficient proof for the correctness of a given view, and require specific experimental and statistical support. The history of medicine is full of examples where assumptions of this kind were almost universally held, only to be disproved a little later. Even in the more exact sciences we need only recall the almost universal assumption that the earth was flat, or that it was in the centre of the universe, to recognize that assumptions, however firmly held, derive no scientific validity from the firmness of the belief of the person holding them.

Altogether then, Davis appears to me to be suggesting a course of action precisely counter to that which is usual in science. Abnormal psychology is an applied science; clinical work generates problems but must for the solution of these problems depend on the pure science of psychology. The application of psychological principles to the explanation of neurotic

disorders and their cures is undoubtedly complex and difficult but not in principle impossible; in *The Dynamics of Anxiety and Hysteria*, I have tried to take some steps in this direction and have given some examples of how this could be done. Davis would appear to want to reverse this process. Starting out with an unproven assumption, he wants vague clinical hunches to determine laboratory investigations, thus putting the cart before the horse in the almost literal meaning of that phrase. He does not at any point give any reasons for reversing the traditional scientific procedure, but appears to base his views entirely on the 'assumption' of therapeutic usefulness of psychoanalytic methods. In arguing his case, as pointed out in the first few paragraphs of this note, he has gone counter to fact in a number of statements, and it must be assumed that, in so far as his argument considers these statements relevant, the fact that they were erroneous must lead him to adopt a position contrary to the one advocated in his paper.

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CLINICAL PROBLEMS AND EXPERIMENTAL RESEARCHES: A COMMENT ON PROFESSOR EYSENCK'S REPLY

By D. RUSSELL DAVIS

In my address I asserted that 'it remains unproven that Pavlovian methods are generally more effective than the many other methods ... which have given impressive results in practice'. I was wrong if the data contained in Wolpe's papers amount to proof. I do not think that they do. Nor did Eysenck, for on page 273 of his monograph he concluded, with admirable caution on that occasion: '... the results as far as they go do not seem to be unfavourable to the Pavlovian type of theory ... no more can be said at present.'

I would withdraw another of my criticisms of the final chapter of his monograph if, as he claims, Wolpe's work dealt precisely with the types of symptoms to which clinicians attach major importance. I do not think that it does. Wolpe describes the application of psychotherapeutic methods based upon his theory of 'reciprocal inhibition' in a series of cases of neurosis. Unfortunately he has told us only in broad outline what he did in each case. Certainly he brought his methods to bear on such symptoms as fears of falling (case 1 in the 1954 paper), obsessional thoughts about food (case 2), total amnesia for 4 days (case 3), phobic reactions to men (case 4) and so on. It appears that he did a great deal more; for example, in case 4 'the greater part of the time' he says, 'was devoted to a discussion of how to gain control of her interpersonal relationships'. Wolpe seems to have been aware of the importance of many of the symptoms with which his special methods do not deal precisely.

I do not wish to suggest that these methods have no value. On the contrary, they are of some interest and appear to provide effective means of removing some types of symptom. It is sometimes worth while, I agree, to treat symptoms directly. It is usually better to treat the disorder which the symptoms reflect. Thus Wolpe treated in his case 4, not mainly the

phobic reactions to men, but a disorder which centred on the patient's relationship with her father; in doing so he appears to have depended upon ordinary methods. To have attempted to bring about the inhibition of the phobic reactions directly might have made an interesting experiment; as treatment it would have been, as I put it in my address, 'to accept too simple a version of the clinical problem'.

However, our dispute about the 'facts' is trivial. Our disagreement about strategy is of more importance, although it is less serious, I think, than Eysenck makes out. Apart from several points which appear to arise from a misunderstanding of my position, Eysenck places the emphasis differently. For instance, it seems to me remarkable that, writing in July 1958, the centenary of the publication of Darwin's theory of evolution, he should maintain that history has shown that success in understanding natural phenomena 'does not usually come from their direct study, but rather from laboratory investigations...'. Did not the results of Darwin's direct studies exert a profound influence upon laboratory workers in their choice of researches, and properly so? Was this to put the cart before the horse?

I agree that the antithesis between clinical problems and laboratory hypotheses is an entirely artificial one, but that it has still to be taken into account in discussing contemporary disagreements between psychologists is made only too plain in Eysenck's reply. Similar disagreements were fought out between physiologists and physicians a century ago. Thus the elements of the disagreement between Eysenck and myself are nearly all discussed by Claude Bernard (1865) under the rubric 'The False Application of Physiology to Medicine'. The passage concludes: 'To sum up, as the natural foundation of experimental medicine, experi-

mental physiology cannot suppress observation of the sick or lessen its importance. Moreover, physiological knowledge is not only indispensable in explaining disease, but it is also necessary to good clinical observation.' This, with obvious substitutions, was the unoriginal theme of my address.

I made the assumption that psychotherapy is effective in a wide range of cases, when it is based on those suppositions of Breuer and Freud which I cited. It is of course normal procedure to express an assumption or a hypothesis in *propter hoc* terms, although the most that one can usually do experimentally is to demonstrate a *post hoc* relationship. My assumption was not an unreasonable one, although unproven. I would not be unwilling to assume that 'magnetic therapy based broadly on the suppositions enunciated by Mesmer' was effective in certain types of case. It might be profitable to ask: through what psychological processes did magnetic therapy bring about its effects? Had I done so, I might well have concluded, as I did about the 'abreaction' hypothesis, that 'if it is to be retained

at all' the 'magnetism' hypothesis 'requires to be expressed in a form more in keeping with contemporary theories'.

Instead, I went on to discuss the processes through which the recall of traumatic experiences brings about its beneficial effect—supposing of course that it brings about a beneficial effect at all. In doing so, I attempted to express more or less 'vague clinical hunches' in a form which would allow them to be tested experimentally either in the clinic or the laboratory. My intention was to meet in a modest way what I called the 'urgent need that clinicians should revise their descriptions of clinical phenomena . . . and put them into a form which facilitates explanation in terms of laboratory theories' (preparing the cart to be pulled by the horse?). Does Eysenck really think, as the last paragraph of his reply seems to indicate, that direct study of this kind is a wrong course of action, counter to that which is usual in science? Has not a similar course yielded valuable results in medicine, both for the pure science and the applied science? Does it not still do so?

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REVIEWS

Therapeutic Education. By GEORGE DEVEREUX.
(Pp. xxviii + 435. 40s.) New York: Harper
Bros. 1956.

Dr Devereux, anthropologist, sociologist and psychoanalyst, is at present Director of Research, the Devereux School, an institution well known in the United States. It is described by him as 'a specialist residential school for children, adolescents and young adults, who, were such a school not available, would have to be placed in conventional mental hospitals or in schools for the feeble-minded and the like'. This book is the result of a study he was asked to make of a number of students who appeared not to have derived benefit from the school's programme of 'dynamic, socialist and individualized education in a therapeutic milieu'. The author states that while pondering over this practical problem he became interested in its formulation as a scientific one, related first to the evaluation of the relative efficacy of educational and milieu therapy compared with psychotherapy carried on in the same setting, and next to the procedure of how treatment could be evaluated not in terms of success but failure.

The preface is devoted to expounding in some detail Dr Devereux's own particular interests and skills, since he believes that to understand the scope, purpose and shortcomings of any book one must have a good deal of information about its author. This he gives us. We are told that the basic conceptual framework of the text represents a summation with reference to therapeutic education 'of a more general frame of reference or scientifically humanistic Weltanschauung, which underlies the writer's published works in a variety of fields, many of which are superficially—but only superficially—unrelated to the focal topic of this book' (pp. xviii–xix).

As an anthropologist, Dr Devereux is interested in the impact of culture on personality formation, and hence in education, for he views all education, including therapeutic, as a branch and application of cultural and personality studies. His interests are also directed towards the relation between variables in cultural setting and personality deviations. He became a psychoanalyst (although by

his own admission a somewhat unorthodox one) in order to carry out diagnosis and treatment more effectively.

He claims that his studies have enabled him to understand with peculiar clarity the therapeutic programme developed to meet the needs of the particular types of children sent to this school, who seem, from the very short case histories, to consist rather largely of feeble-minded, including brain-injured, epileptic and psychotic, as well as a certain number of (presumably) severe behaviour disturbances of one kind or another. Indeed, he goes so far as to say that anthropology is an indispensable tool for anyone who wishes to construct a really effective therapeutic milieu genuinely adapted to the needs and potentialities of certain categories of exceptional children and adults. He bases this conclusion on the idea that such deviates approximate in some respects to certain kinds of primitive peoples, stressing the fact that protective groups (such as this particular school represents) are in size 'about that of a native tribe, while the complexity of their social structure and of their culture is comparable to the complexity of a primitive society and primitive culture' (p. 275). (The fact that this school has grown in size from two to its present population of over 600 prompts the query as to the period in which this approximation could be deemed most complete.)

It is a pity that amongst his other studies Dr Devereux appears not to have included either the psychology of the normal child or educational psychology, or that he seems not ever to have been a school teacher, since some of his statements regarding the relations of children to each other in educational groups, and of the parent-teacher relationships, show the effects of gaps in such knowledge.

The book has the following aims: to establish a nexus between the fields of therapeutic education, education and psychotherapy on the one hand and the broad social and cultural context in which these disciplines are implemented on the other; to air some of the problems which confront those entering the field of therapeutic education; to interpret the work of the educator to the therapist and vice versa, with special reference to the organizational

setting in which interaction between the two takes place.

It is by no means easy to decide to what extent Dr Devereux has succeeded in achieving these aims. One difficulty confronting the reader is purely linguistic. Dr Devereux is not one to avoid complexity of language, whether it mirrors complex or only obscure thoughts. He has a fondness for coining and employing new words which do not always express new ideas. On a more trivial issue the multilingual phrases scattered lavishly throughout the text do little to clarify the points made, and the use of so many footnotes does not always seem justified.

The reader who has little knowledge of anthropology may well find himself at a loss. Illustrative examples are taken from a wide variety of cultural and ideational frameworks. In the section dealing with basic principles, for instance, the reader in the course of a few pages is transported at high speed from the medicine men of the Sedang Moi to the aborigines of Australia, to the Maori, the Sioux, the Greeks, the Mohave Indians and the ordinary American citizen. Even when discussing the managerial revolution and its connexion with the organization of schools and hospitals, Dr Devereux feels impelled to bring in the head-hunters of the Naga culture, presumably to illustrate the point that all societies have their own systems of values. The reader without precise anthropological knowledge, who may not have read Dr Devereux's other publications in which he talks at more length about his *Weltanschauung*, will find himself at a disadvantage. In fact he may only be able to cope with this in the same way as the small boy, who, when being assaulted by an over-enthusiastic parent, may console himself with the reflexion that 'this is doing you more good than it's doing me'. I am sure it did Dr Devereux good to get it out of his system: I am less sure that the teacher intending to enter the field of therapeutic education will either be able to follow, or derive benefit from, this kind of approach.

This is a pity, since much of what Dr Devereux has to say is of interest, and a good deal of it is characterized by commonsense and practical realism oddly at variance with some sections of the book.

The text is divided into the following sections: Part I, basic principles—the meaning of education for development and a historical sketch of the

nature and scope of education; Part II, therapeutic education and psychotherapy, including discussions on the different nature of learning in both these techniques, the differential tasks of therapeutic education and the overlap between the two areas; Part III, problems of socialization and discipline, related to the concepts of ego and super-ego in the first place, and its connexion with socialization and discipline, and finally problems connected with teacher-child and peer relationships in protective groups. In Part IV he deals with the organizational pattern of therapeutic institutions and the problems that arise from the fact that people in different disciplines have to work together in such a setting, while Part V is devoted to a consideration of obstacles in the way of therapeutic education.

This is a spacious framework in which to expand his general theoretical principles and to formulate procedures for practical therapeutic education and administration. With such a framework it is inevitable that many ideas are mentioned without being adequately developed. For instance, much of what he has to say about basic principles of education and his general sketch of the history of education show the dangers inherent in attempting to pack too much into too small a space.

In the first section Dr Devereux draws a distinction fundamental to the later exposition between instruction, or the teaching of specialized techniques which takes no account of the value system of the society in which it is carried out, indoctrination, which he takes to be the teaching of social techniques with reference to the special value systems of the society, and education, a creative integration of instruction and indoctrination. It is the task of the therapeutic school to carry out the third. He makes a further distinction between true psychotherapy, remedial measures and therapeutic education. The aim of remedial techniques is to correct inadequate activities relating to the (usually conscious) ego functions by a direct attack on the symptoms themselves, as distinct from the aims and techniques of psychotherapy. Therapeutic education has much in common with both of these contrasting techniques, and he devotes a great deal of the book towards elaborating points of similarity and difference, much of the exposition being on a somewhat abstract level. Of practical interest to the intending teacher are the sections in which he gives an account of factors to be taken into consideration in dealing with cases

—factors relating to age of child, length of duration of symptoms and so on — which will enable the educational therapist to estimate probable success of treatment. Dr Devereux also comments sensibly on the advisability of considering the abilities of deviates in relation to the use which he himself is making of them, and not primarily in relation to society in general. He has also something to say about the problems which confront the educational therapist. First, those which arise in relation to the psychotherapist. Unlike the latter, the educational therapist has the task of coping with the behaviour of the child in day-to-day situations (being in this respect rather like the parent whose child is undergoing psychotherapy in an out-patient clinic). He also knows a good deal more about the child in many important respects than does the psychotherapist, and yet professionally he finds himself regarded at a lower level. It is not only in this relationship that difficulties may arise for the therapeutic teacher: he has to deal with parents. Dr Devereux attempts to show that where teachers have taken over the task of indoctrination they have hostility to encounter from the parents, and he draws on numerous examples from the cultures of Greece, Rome, France and other countries to support his point.

As indicated above, one real difficulty in attempting to assimilate the material contained in this text is that points are linked to such a variety of illustrative statements from so many different sources, that the attention of the reader is diverted from many of the teaching points made. The case histories, unfortunately, are in general rather poor, being so short and general in statement that their aptness for illustration is in many cases better appreciated by the author than by the reader. The most practical sections of the text are those devoted to the problem of success and failure in therapy and education (ch. 7) and parts of chapters 8 and 9 on socialization, discipline and punishment. Here the case histories are much more satisfactory and aptly illustrate the points he is trying to make.

To begin to summarize the other themes developed in the course of the volume is a difficult task. One feels one would like a quick guide to essential points. The task of coping not only with Dr Devereux's style but his *Weltanschauung* at the same time has effectively prevented one reader from attempting to compile such a guide.

G. H. KEIR

Psychology of Exceptional Children and Youth.
 Edited by WILLIAM M. CRUICKSHANK.
 (Pp. v+594. 50s.) London: Staples Press
 Limited. 1956.

This book, first published in the United States in 1955, is a welcome and valuable addition to specialist literature on the exceptional child. In the preface, the editor describes it as 'a symposium prepared by psychologists, devoted solely to the psychological considerations of the influence of physical deviation upon the normative growth and development of children and young people'. Almost at once, however, he defines the exceptional child as one who, by reason of a physical or intellectual deviation, is considered unique among children. Hence, there are included two chapters on intellectual deviates—what, in this country, would be called the intellectually subnormal and superior child. Logically, one supposes, such a symposium might also have contained a contribution on the psychology of children with marked superior physical development or physical skills, but the psychological repercussions of these have as yet not attracted any degree of study.

The bulk of the book is made up of studies in the psychology of children suffering from various types of physical handicaps—impaired hearing, defective speech, impaired vision, and the crippled. In considering the problems of the crippled child, special attention is devoted to problems connected with cerebral palsy, epilepsy, heart conditions, diabetes, endocrine disturbances, allergies and the like. This very considerable body of data represents the most comprehensive compilation and discussion of fact and theory related to the psychology of the physically handicapped which has yet appeared in the literature. Each contributor has obviously made a thorough survey of much of the literature relating to research in his own field, as is evidenced by the references throughout the text. The result, however, is not a mere source book or rapid survey of the types sometimes found. Each reference has its particular function in developing a point to be made, and the volume as a whole is a well-organized and excellently presented body of knowledge in all of these respective fields—no mean achievement. The authors must be congratulated on the fact that they have taken the trouble to make themselves familiar with work carried out in other countries, although there are some rather curious omissions. For instance, the

publication of the National Foundation for Educational Research on the educability of cerebral palsied children does not appear to be included among the references. The checking of references, however, is not made easier by the fact that authors are not arranged in alphabetical order.

The authors of the eleven chapters seem to have been left free to treat their subjects in any fashion they please, although naturally the scope of the book ensures a good deal of uniformity of content and treatment. Three chapters, two by Lee Meyerson on the Somatopsychology of Physical Disability and A Psychology of Impaired Hearing, and one by the editor on Psychological Considerations with Crippled Children, are written from the Lewinian point of view. It depends on one's own psychological point of view whether such a treatment really provides anything more than a rather more complex mode of description. It remains doubtful as to what extent it can deal with genotypal factors. Apart from these three chapters, the rest of the volume is presented in the form of discussion related to data obtained from research, not interpreted in terms of one theory of human behaviour. As indicated above, there is considerable variety in the treatment of each topic. Thus, the chapter on Defective Speech by Jon Eisonson is confined mainly to a straightforward presentation of the types, incidence and causes of speech defects, their relation to level of intelligence and reading ability, and the connexion between speech defects and other motor and sensory defects. Perhaps the shortness of the section on psychological conditions reflects the direction of current interest and knowledge rather than the preferences of the author. At the same time, to devote rather less than half of the entire chapter to the discussion of psychological conditions, and of this to give only four pages to the adjustment of stutters, does not do full justice to the complexity of this form of speech defect.

The chapters on children with impaired hearing and impaired vision fulfil admirably the purpose of the volume. The latter is especially informative. The sections on the cognitive functions of vision, the growth of experience of the object world, the problems of the development of concepts of space, form and colour among the blind, could well be read, not only by the psychologist interested in such handicapped children but also by all who are interested in the problem of how individuals come to know their world. The effects of blindness

upon personality adjustment are well discussed and include a mention of the important field of sex adjustment. This is either omitted or given a rather cursory treatment in other chapters. Even in that dealing with the problem of the deaf, it is somewhat briefly discussed and summed up in the following statement, that 'they love, work and play in ways which do not appear dynamically different from the ways others do these things' (p. 156).

The chapter on the psychological problems of crippled children contains some useful comments on the vexed question of how to test effectively the intelligence of children suffering from cerebral palsy, which complements the discussion on this point contained in the chapter on the Psychological Assessment of Exceptional Children and Youth, by Ernest Newland. The section on the social adjustment of crippled children is a little less satisfactory, for the author states as a broad general premise that 'the basic adjustment problems of the crippled child are the same as those of children of comparative chronological and mental development who are physically normal' (p. 285). He states that 'the physically handicapped child in his social relationships is, as are all children, attempting to ensure not his physical organic self, but his phenomenal self, the concept of himself of which he is cognisant'. Developing this in Lewinian terms, Dr Cruickshank goes on to say that the adjustment of the crippled child involves two sorts of problems—'those which might occur in the normal development of any individual who is simultaneously striving for separation of the self and for the maintenance of the self concept as already developed, and those special adjustment problems which are solely resultant from the fact that a physical handicap is inserted between the goal and the self desire to achieve such a goal' (p. 285). His first statement by its breadth may lead to an over-emphasis on adjustment connected with developmental growth. However, the point is well taken that perhaps over-much emphasis has been placed on the second type of problem, with a consequent neglect of the first. The extent to which the second is operative in determining the ultimate behaviour of the crippled child does depend upon the psychological attitudes developed towards such frustrating factors, but nevertheless the statement: 'If the child conceives his physical disability as a barrier, organically or psychologically, to satisfactory adjustment being attempted' (p. 286), seems to pay too little attention to the

fact that, where the crippling condition is very serious, the task of adjustment may prove unusually difficult. A discussion on the value of special environments as an aid to such adjustment would have been of the utmost value. Although there are sections on relations with parents, acceptance or rejection by them being considered as well as parental rearing practices, there is almost no mention made of the contributions which educational environment and treatment make towards the child's capacity to deal with problems of this second type.

Indeed, this is a subject to which rather little attention is directed in the volume as a whole. There is a wide variety of different practices concerned with the education of the physically handicapped child, especially in the United States, and a number of important problems arises in connexion with the relative efficacy of different procedures. It would have been valuable to have had space allotted to these, for school plays a great part in the adjustment process of children. Perhaps a volume on the psychological effects of different educational procedures will in due course make its appearance.

The chapters on the psychology of the intellectually subnormal and superior child are useful, although much of the material can be found in other text-books. The final chapter on psychotherapy is stimulating, since, as the author states, 'the scientific study of the use of psychotherapy with the disabled is presently in its earliest state' (p. 567). He draws attention to the important and neglected field of psychotherapy with children having sensory defects. He discusses the results of therapy with other classes of physically and mentally handicapped children. Not the least interesting is that section devoted to the mentally handicapped child in psychotherapy.

As stated at the outset, this book satisfies a long-felt want and does it in a fashion which is almost wholly admirable. It is, or should be, indispensable not only to the worker in these special areas but also to all psychologists interested in the problems of child development.

G. H. KEIR

New Developments in Analytical Psychology. By MICHAEL FORDHAM. Foreword by C. G. JUNG. (Pp. 214. 25s.) London: Routledge and Kegan Paul. 1957.

This mature and thoughtful contribution, published thirteen years after *The Life of Childhood*,

ranges over a wide variety of topics of practical and theoretical interest. Eight of the nine chapters are based on papers read during the intervening years to a number of audiences, whilst the longest chapter of the book, 'Notes on the Transference' is the most significant as far as new developments in analytical psychology are concerned. It constitutes the first survey of the various trends in a field which had been neglected by the earlier followers of Jung and is a lucid exposition of the author's views. The modest title of the chapter presumably indicates that the last word on this topic has not yet been written! At any rate Fordham's views are derived from his pioneering work in child psychology to which indeed over a third of the pages of the present volume are devoted. It is characteristic of his writing that his theoretical formulations are at all points closely connected with his clinical observations. This makes the book very readable.

Chapters like 'The Origins of the Ego in Childhood' go a long way in giving analytical psychology much needed and detailed ontogenetic foundations. 'Biological Theory and the Concept of Archetypes' should dispel once and for all the notion that analytical psychology is a philosophy devoid of biological and genetic roots. Indeed the Jungian reader may well be amazed at Fordham's feat of expanding the boundaries of analytical psychology while remaining at the same time firmly based on the originator of this 'school'.

The book has an inner consistency, and to quote from Jung's Foreword 'is distinguished by its far-sightedness, carefulness and clarity of style' which will assuredly put it among the most important contributions to analytical psychology. The topics covered by Fordham's book make comparisons with 'new directions' in psychoanalytical writings feasible.

A. PLAUT

The Normal Child. By R. S. Illingworth. 2nd edition. (Pp. 356. 35s.) London: A. and S. Churchill. 1957.

Now that physically-ill children are becoming a 'dying' population, paediatricians are turning their attention more and more to psychological disturbances, thus encroaching on the jealously guarded preserves of the child psychiatrist. It is even conceivable that, in the not too distant future, they will 'incorporate' the latter and earn a new lease of life as 'psycho-paediatricians'. As

a child psychiatrist myself, it is understandable that I would prefer to eat Dr Illingworth up than vice versa, but I do not think that the time is as yet ripe for such psychic cannibalism. For the time being at any rate we can defer the battle for possession of the child, admitting each other's predominant experience, interest and skill.

Those, however, who favour the emergence of a portmanteau-specialist should be delighted with this book, since the author appears to be equally at home in both spheres. He offers excellent medical advice while at the same time planting both his psychological feet firmly on the Gesselian schedules. I hope that in the next edition he will expand his conceptual frame of reference and bring in Freud and Piaget, who have so many more significant things to say about the normal child.

What I like particularly about Dr Illingworth is that he sits on no fences. For example, in the breast against bottle controversy he comes down so strongly on the side of the more hygienic and labour-saving human containers that the infant lying dormant in the minds of all his readers will rejoice at the victory.

Feeling myself (as yet) unthreatened by this new comprehensive discipline of child health, I can recommend this book to all students interested in the range of normality. Analysts should profitably learn something about the real developmental stresses that the child has to endure in addition to his phantasies.

E. J. ANTHONY

Perceptanalysis—A Fundamentally Re-Worked, Expanded and Systematized Rorschach Method. By Z. A. PIOTROWSKI. (Pp. 474. £2. 7s. 0d.) New York: The Macmillan Company. 1957.

For a long period Dr Piotrowski's contributions to various journals have commanded the respect of all serious students of personality. On the Rorschach method in particular he has directed a mind that is highly critical and scientifically orientated. In this book, published after twenty-two years of research, justifiably he writes as one having authority. He makes it clear that there is no easy road to an effective use of the method demonstrated. 'At least two years of closely controlled and supervised training are indispensable for mastery of the technique'; 'Most of the dis-

agreements about perceptanalysis are due to misunderstanding or ignorance', and, it might be added, to frustration, since such a tool, if unskillfully used, will produce poor results and consequent anger.

The defined purpose of this book is 'primarily to contribute to a more productive and more systematic interpretation of perceptanalytic Rorschach data', such data being analysed in terms of the perceptual process apparent. This emphasis upon analysis of perception, though it has long been the basis of the 'Tavistock' Rorschach teaching in Britain, yet makes Piotrowski's work unique among the profusion of literature on projective testing. The evidence of perceptual samples is no less objective than that of physical samples, and like the latter can be subjected to scientific methods of assessment. Piotrowski has the courage to state that his method is a science, but he does not make such a claim lightly, nor does he make the error of supposing that the word 'science' has an equal and precise meaning for all who use it. His chapter 'Perceptanalysis as a Science' first examines the concept of science in terms of principles, empirical referents and validation before applying these criteria to his systemized method.

Technically, especially for those trained in the Rorschach, there is an enormous amount of valuable material, clearly set out, closely reasoned, with much original thinking yet extraordinarily wide knowledge of the work of others in many fields. So much is condensed in one volume that it is not easy to digest, but Dr Piotrowski is a wise humanist as well as a scientist, and he is never dull. On the background of his vast clinical experience he can afford to be sometimes dogmatic, often controversial: in these ways he teaches the better by stimulating the reader to think for himself. He has produced one of the most valuable books ever written on the Rorschach method of personality diagnosis. It has been long awaited, and amply justifies the time of its preparation.

THEODORA ALCOCK

The People of Ship Street. By MADELINE KERR. The International Library of Sociology and Social Reconstruction. (Pp. 215. 23s.). London: Routledge and Kegan Paul. 1958.

Visiting scientists are often made quickly aware that two of the main areas where work of out-

standing value is done in Britain are in the systematic study of social structure and in the understanding of personality. It is perhaps less obvious that the extent to which scholars and practitioners in these two fields actively relate themselves to each other is small. There are many reasons for this. Professional competence in either field demands years of sustained intellectual effort and strenuous first-hand experience. Few can afford the time and expense for both forms of training. To some extent also, different kinds of people are attracted to these two kinds of discipline; some would say that while one kind eschews emotion the other makes a virtue of untestable intuition. Whatever the reasons, there are few competent sociologists in Britain who have what a specialist in personality would regard as a reasonable working knowledge of personality while the propositions of the latter about environmental forces make the social scientist wince. It may be that we will have to reconcile ourselves to the continuation of this position. Indeed some influential academic quarters are against the crossing of the line on the grounds that good work is unlikely to be done in either field by scholars faced with frames of reference which might be complementary in different people but can be conflicting and confusing within one person. There seems much to justify this view. However, experience demonstrates that social scientists and 'dynamic' psychologists find it extremely difficult to collaborate in research unless each has proceeded well beyond defensive mutual contempt and beyond lip-service through a genuine working experience to a real internalisation of something of the other's outlook and skills.

The next generation will contain a higher proportion of psychoanalytically trained social scientists and of psychiatrists and psychoanalysts with a social structural component in their training. This should help to make it easier for each group to take the contributions of the other seriously and for understanding to break across the highly formal divisions between disciplines which are necessary for rigorous formulations but also distract attention from the complex and multi-level interplay of societies, their subgroups and their individual members.

In the meantime we should be grateful for the few pieces of work like 'The People of Ship Street' which attempt to develop a new field on the boundaries of sociology and dynamic psychology.

This book, written by a social psychologist experienced in field research in the two contrasting social structures of Jamaica and Britain and with special training in projective techniques, attempts to formulate hypotheses about the social origin of immaturity, violence and certain adolescent difficulties. The author describes a Liverpool Irish community which is split up into closely knit family groups each dominated by a matriarch and follows this with the tentative proposition that 'the social psychological conditions under which these people live do not allow them to develop sufficient ego strength to cope with anything other than the very limited conditions under which they live'. By 'social psychological conditions' she means much more than unconscious interplay between personalities—she is taking into full account the social roles of the individuals involved, the rights and obligations which hold them together in groupings and networks and the values and ideologies in terms of which they live their lives.

In so far as the book is an attempt to derive explanations of the structure of characteristic individual personalities from the social structure which contains them, it contains much that can be faulted either by captious sociologists or psychologists. The social structural analysis could have been driven through more relentlessly and enriched by more systematic and conscious use of reference group social network conceptions. Miss Kerr could usefully have pushed the comparison of her Jamaican and Liverpool material much further. On the psychological side the descriptions of personality seem to lack a certain depth and movement. The central concept of a personality basic to a community is itself still suspect.

These criticisms of the limitations of the book are, however, of less importance than the contribution which Miss Kerr is making to work attempting to synthesise different categories of thinking, an effort which is essential if ever we are to grasp the real unity of social and psychological phenomena.

CYRIL SOFER

The Analysis of Dreams. By MEDARD BOSS. Translated by ARNOLD J. POMERANS. (Pp. 223. 25s.) London: Rider. 1957.

Dr Boss, a distinguished psychoanalyst and Prof. of Psychotherapy at Zurich, presents a so-called phenomenological approach to dreams inspired

largely by the work of Martin Heidegger. He sees in *all* dream theories hitherto (he examines in particular the theories of Freud, Jung, Maeder, Fromm, Schultz-Hencke, Leonard, Bossard and Klages), a tendency to replace the actual dream phenomenon by explanations of it; in one way or another the concrete phenomena are made to take second place to hypothetical underlying mechanisms or agencies. All the dream elements, its time and place, people and things, are always seen as the expressions of something other than what they actually are in the dream. Either they are 'interpreted', or 'explained' with reference to inner forces at work in the psyche (dream interpretation on the subjective level), or they are held to be symbolic disguises or representations of real external objects (dream interpretation on the objective level). In either case the 'cause' of the dream is looked for in hypothetical abstractions, whether these be unconscious drives or inferred archetypes, whereas the 'essence' of the dream itself is overlooked. All such explanations are held by Boss to be based on the model of natural science, and to show 'a contempt for and abuse of the immediate reality' of the concrete phenomena of the manifest dream as it is in itself. In psychotherapy, however, he thinks that interpretations on the subjective or objective level can be effective, and may even be indicated, if the patient is not able to tolerate the full realization of his responsibility for the existential situation which, in the dream, he 'really' is in.

Boss himself proposes 'to forgo all theories and hypotheses, to concentrate on the dream itself and to see what we can learn from it alone' (p. 9). He sees his task as the liberation of the dream from the shadow of every point of view, psychological, anthropological, natural scientific, which would see the dream as the expression of anything else, and, thus, to 'place it in its own light' (p. 10).

Basing himself on Heidegger, and taking him as more or less read, Boss goes on to attempt what has in fact never been done before, at least systematically, namely a phenomenological analysis of dreams; that is, to analyse them 'in terms of their own nature and without theoretical suppositions and reductions' (p. 80) and without doing violence to them from the perspective of waking life. He cites dreams to illustrate the range of human possibilities in the dream—dreams that may be held to support the view that we can, for instance, while dreaming, reflect, tell lies, have

conscious thoughts, make artistic and moral judgements and so on. He shows that a great range of relationships to persons and things can occur in dreams. He pays particular attention to the phenomena of space and time, magical and 'non-sensical' relationships.

Boss displays an invigorating verve in polemics, but he lacks that infinite patience with which he credits Freud, for the painstaking philosophical task of working out his position with consistency and following it through with rigour. His worst philosophical enemies would hardly deny such patience, consistency and rigour to Heidegger, but unfortunately the careful meticulousness of 'Sein and Zeit' cannot save Boss from his own indiscretions. He recklessly writes off Heraclitus and Husserl in a sentence each. Binswanger rates half a page. Sartre, who has a closely reasoned chapter on the phenomenology of the dream in his 'Psychology of Imagination', radically at odds with Boss on many points, is not mentioned. While refusing to allow any perspective from waking life to 'distort' his account of the way we exist in dreams, he fails to show how if this is his position, he can describe such an entity as a dream at all, for the judgement that we dream is a waking life judgement and a precondition for his writing his book. He asks, with the Chinese sage, how, having dreamt I am a butterfly, do I now know that I am not a butterfly dreaming I am a man? Boss does not mention that Descartes states the identical problem in principle in his first Meditation, which, considering Heidegger's relation to Descartes, might have warned him that he had perhaps taken a wrong step somewhere. For Boss seems to have slipped into a solipsistic position by discarding Heraclitus' basic distinction between the private world of the dream and the shared world of waking life, on the grounds that 'man is very rarely alone in his dreams' (p. 89). When a man was bitten by a dog in a dream, this dog is 'neither a reproduction nor a symbol, but was from the very beginning a very real and most lively dog' (p. 110). He speaks of a dreamer experiencing 'with all her body and soul a world as completely real as she had ever felt during waking life' (p. 82). In the dream 'we are always within a world the reality of which we had best not deny too hastily' (p. 85). 'In our dreams we experience real physical facts: a thing is a real thing, an animal is a real animal, a man is a real man, a ghost is a real ghost. In our dreams we are in

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Boss displays an invigorating verve in polemics, but he lacks that infinite patience with which he credits Freud, for the painstaking philosophical task of working out his position with consistency and following it through with rigour. His worst philosophical enemies would hardly deny such patience, consistency and rigour to Heidegger, but unfortunately the careful meticulousness of 'Sein and Zeit' cannot save Boss from his own indiscretions. He recklessly writes off Heraclitus and Husserl in a sentence each. Binswanger rates half a page. Sartre, who has a closely reasoned chapter on the phenomenology of the dream in his 'Psychology of Imagination', radically at odds with Boss on many points, is not mentioned. While refusing to allow any perspective from waking life to 'distort' his account of the way we exist in dreams, he fails to show how if this is his position, he can describe such an entity as a dream at all, for the judgement that we dream is a waking life judgement and a precondition for his writing his book. He asks, with the Chinese sage, how, having dreamt I am a butterfly, do I now know that I am not a butterfly dreaming I am a man? Boss does not mention that Descartes states the identical problem in principle in his first Meditation, which, considering Heidegger's relation to Descartes, might have warned him that he had perhaps taken a wrong step somewhere. For Boss seems to have slipped into a solipsistic position by discarding Heraclitus' basic distinction between the private world of the dream and the shared world of waking life, on the grounds that 'man is very rarely alone in his dreams' (p. 89). When a man was bitten by a dog in a dream, this dog is 'neither a reproduction nor a symbol, but was from the very beginning a very real and most lively dog' (p. 110). He speaks of a dreamer experiencing 'with all her body and soul a world as completely real as she had ever felt during waking life' (p. 82). In the dream 'we are always within a world the reality of which we had best not deny too hastily' (p. 85). 'In our dreams we experience real physical facts: a thing is a real thing, an animal is a real animal, a man is a real man, a ghost is a real ghost. In our dreams we are in

just as real a material world as in our waking life...' (p. 106).

Read in one way, this may be felt to be simply silly, but here I think a more literal translation would have gone some way to obviate this impression. I do not know whether the translation is authorized, but it does not always follow the original very closely. A more literal translation, for instance, of the original of the last passage (with my italics) is: 'Dreamers experience the dream phenomena, *while dreaming*, as real physical data: a thing *as a real thing*, an animal *as a real animal*, a man *as a real man*, a ghost *as a real ghost*. *In our dreams* we are in a world as genuine and graspable as in our waking life...'. Although these 'slight' differences do, I think, put a more 'common-sense' complexion on this passage, Boss does seem to have got himself into a position in which he lacks any adequate criteria for distinguishing the nature of the actuality of the dream from the actuality of the world.

Boss, who has of course great clinical experience of his subject, adduces many fascinating dreams, some of which he relates to the total life situation of the dreamer in a most convincing way. It is necessary to note, however, that Boss himself by no means confines himself to so-called 'pure' phenomenological analysis of dreams, but gives many interpretations. He 'explains' how and why dreams occur by innumerable phrases such as 'that is why', 'this resulted in', 'the dreamer did this because...', which are by no means 'given' in the dream phenomena themselves. Yet it is just these very interpretations, some of which are very wise, and the dreams themselves, some of which are genuine collectors' pieces, that are among the most valuable assets of this extraordinary book.

R. D. LAING

Mastery of Stress. By DANIEL H. FUNKENSTEIN, STANLEY H. KING AND MARGARET E. DROLETTE. (Pp. xv+329. 48s.) Harvard University Press; London: Oxford University Press. 1957.

This book is about physiological and psychological tests performed on healthy Harvard students to study their acute reactions to frustration and their ability to handle frustration over a period of time. Three acute reactions are described, anger directed outwards, anger directed inwards and anxiety. They found that there was

no relationship between the type of acute reaction and whether or not the students were able to handle the stress as time passed. The physiological tests showed that those students whose acute reaction was anger directed outwards had evidence of excessive secretion of a *nor*-adrenalin-like substance, while those who reacted with anger directed inwards or with anxiety showed evidence of excessive secretion of an adrenalin-like substance. These findings are in accord with neurophysiological advances since Cannon's description of his fight/flight reactions. The batteries of psychological tests have been subjected to statistical analysis and the ability to master frustration has been correlated with ego functions.

As very general hypotheses are made from this detailed and diligent laboratory study two comments seem relevant. First, only frustration has been selected for investigation and the assumption is made that the reaction to all stresses is the same. Whereas on a broadly statistical basis this may be true, this global approach glosses over all the finer and indeed meaningful nuances of feelings and phantasies. Secondly, no allowance is made for the role of unconscious needs in producing situations of stress. The contribution to the study of reactions to frustration made by this book is a modest one.

ALEXIS BROOK

Great Men: Psychoanalytic Studies. By EDWARD HITSCHMANN. (Pp. xiii+278. \$4.00.) New York: International Universities Press, Inc. 1956.

The bibliography of the writings of Dr Eduard Hitschmann (1871-1957), which is to be found at the end of this volume, attests both the length of time in which he has been concerned with psychoanalysis and the range of his interests in its applications. He was in fact one of the first to investigate the figures of the past with the help of findings gained from the living; and the present book gathers together several of the more important of Hitschmann's biographical studies, some of which appear for the first time in English.

Amongst other papers this volume contains studies of Schopenhauer, Goethe, Eckermann, Samuel Johnson, Boswell and Brahms, as well as a section on 'New Varieties of Religious Experience'. A too discursive introductory essay attempts to lend the book some unity. In the course of it Hitschmann remarks that he was led

to investigate famous creative individuals in part by the hope of discovering the origins of the creative imagination. Readers who share that hope are likely to be disappointed, as the author must have recognized; such uniformities as can be established here for the infantile experience of writers and artists seem altogether too tenuous. More promising is the examination of the motives which lead to a writer's choice of subject-matter. One of Hitschmann's 'great men' is a biographer and one can be loosely so called; their parasitic relation to their subjects is deftly explored by the psycho-analytic biographer.

Much the most considerable is the study of Schopenhauer; it amounts to a small book in itself. Its convincing quality may indeed derive in part from its length. Hitschmann allows himself the space fully to describe and discuss what is known of Schopenhauer's family and character before proceeding to a critique of his philosophical system. He has then little difficulty in establishing the relations between the former and the latter: in demonstrating the projection into metaphysics of the philosopher's own (meta) psychology. Hitschmann's general animadversions on philosophers are probably sound enough if we take them as applying to *system-building* philosophers. But it is as well to remember that there have been and are other kinds; many indeed have explicitly disclaimed all intention of defining the nature and purpose of the cosmos. These would not appear to be so exposed to the charge of fashioning the world after their own image.

Freud is known to have thought highly of Hitschmann's biographical writings. They are indeed tastefully written, are easy to read, and genuinely illuminate aspects of the great figures that they study. But too much should not perhaps be claimed for, or expected, from this form of investigation. Starting as the handmaid of biography proper it may too readily degenerate into a kind of *belles lettres*. The validation of interpretations and reconstructions, hard enough in the circumstances of daily contact with a living subject, must become even more fragile in the case of a historical figure where valuable evidence has been removed by many forces other than repression; and speculation stimulated by a writer's finished productions is a very inadequate substitute for direct access to his infantile memories, dreams or transference behaviour. It is therefore

a matter for regret that Hitschmann at no point makes any attempt to define the criteria which should govern the writing of such biographical studies, or the range of their possibilities and their limitations.

ALAN TYSON

The Legacy of Sigmund Freud. By JACOB A. ARLOW. (\$2.00.) New York: International Universities Press. 1956.

This short book is in the nature of a centenary tribute, as one might perhaps guess from its title. It consists of eleven chapters, most of them only a few pages in length, which attempt to survey and to summarize Freud's chief contributions to science and to the advancement of understanding in the various fields which attracted his interest. In addition to reviewing Freud's biological, psychological and cultural researches it includes sections on 'Freud as a Translator' and 'Freud's Literary Style'. Criticism is scarcely attempted; and there is in fact little here which could not be found set out more fully in Ernest Jones' *Life* or with more authority in Freud's own expository writings. The aim of the present monograph has rather been to present the richness of Freud's genius and the range and variety of his interests. This has been done neatly, and—within the limits which brevity imposes—accurately. ALAN TYSON

Theories of Personality. By C. S. HALL and G. LINDZEY. (Pp. 572. 52s.) London: John Wiley. 1957.

The aim of this book is to survey the main theories of personality in sufficient detail to give the reader a balanced view of the scope and content of the theory and the place which each theory holds in the constellation of personality theories. This aim it achieves magnificently. Seventeen main theories are reviewed and a number of subsidiary but related ones.

Many readers of such review books cavil at the selection of theories which are made; they query the credentials of some of the authors included, and bemoan the omission of others. Readers of this *Journal* will regret the omission of any reference to Melanie Klein and the Object-Relations school. And in other countries other omissions will be regretted; for example, European phenomenology and Existentialism. Hall and Lindzey have written the book from the point

of view of the main current interests of Academic Psychologists in the U.S.A., and, for that matter, the current interests of their colleagues in Britain and the entire English-speaking world. Their book will become one of the most ubiquitous university texts; it fills a large gap in the psychological literature and it does so in a masterly fashion. Each chapter is the result of careful research, and Hall and Lindzey are to be congratulated for departing from the usual stereotypes about each author which have been established on the basis of their earlier work. They have incorporated the later writings of each theorist and tried to integrate the author's works into as mature a theory as possible. Their integration has been aided by the fact that eleven of the authors have actually checked the manuscript of the relevant chapter.

What is the value of this book to the practising medical psychologist? That is up to him. There is no theory in the book which could not conceivably have considerable relevance to diagnosis and therapy, but the practitioner who has concentrated on the one or two personality theories that form the basis of his thinking is unlikely to find much elucidation of these favourite theories between the covers of this necessarily limited collection of treatments. But if he wants suggestive leads to theories that are worth delving into in more detail, he cannot do better than to read this authoritative work.

RONALD TAFT

Towards a Measure of Man: The Frontiers of Normal Adjustment. By PAUL HALMOS. (Pp. viii+250. 28s.) London: Routledge and Kegan Paul. 1957.

The central question to which the author addresses himself is the nature of psychological normality and abnormality. In seeking the answer he takes the reader on a guided tour of the fields of biology, psychology, psychoanalysis, social philosophy, politics, aesthetics and theology. The tour is perforce a rapid one, but the view is worthwhile; we glimpse other fields, a wide vista is provided, and yet the path we follow is reasonably straight. There is a pleasing integration of the different disciplines, and we meet stimulating arguments *en route*. But when all is over the reader will surely wonder whether he has been brought to the destination intended by the author.

In fairness, it must be admitted that the question with which the book attempts to deal is a difficult

one, and the existing intensive literature on the subject is for the most part superficial and narrow. Halmos, with his knowledge of many disciplines, provides us with a synoptic understanding of human behaviour which would be impossible without his unusual interdisciplinary knowledge.

Halmos's case, in brief, is that we cannot define normality but we can establish its frontiers by defining abnormality. Abnormality 'obtains when the individual suffers social-sexual frustrations in any of a number of specified experiences listed in an inventory universally agreed upon, provided these experiences are followed by any of a number of specified manifestations listed in a similarly accepted inventory' (pp. 80-81). The author constantly refers to his primary and secondary inventories of abnormal adjustment conditions, but unhappily the first turns out to be a fragment only and the second merely programmatic. Nor is the universal agreement documented in any satisfactory fashion.

The criterion of normality provided by Halmos is ultimately a metaphysical one, a model of the conflicting principles of *growth* and *cohesion*—he compares these in an interesting way with Eros and Thanatos. These principles are basic to the behaviour of all living matter and in humans the conflict between them leads to a disbalance which may be 'serviceable' or 'unserviceable'. Halmos's treatment of this central theme, and his discussion of specific points, are rewarding, even though the medical psychologist will not be provided with the ultimate criteria of normality-abnormality. It may, however, help the psychologist to find these criteria for himself.

RONALD TAFT

Mental Health in College and University. By DANA L. FARNSWORTH. (Pp. 244. £2.) Harvard University Press; London: Oxford University Press. 1957.

It is nowadays well accepted that psychiatric services should maintain close links with the education of children, but there is no such general feeling about the relation of psychiatry to the education of students at the universities.

This book, by a Professor of Hygiene who is also Director of the Health Services at Harvard University, is essentially a plea for more psychiatric assistance and for more emphasis on mental health in colleges and universities. Dr Farnsworth recommends that a university should introduce

a mental health programme, under the direction of a psychiatrist or a psychologist, as a normal part of its health services. He advocates not only that there should be adequate provision of psychotherapy and counselling but that the programme should extend far beyond the limits of the clinic, and directly influence the entire academic community. It would be concerned with discipline—'preferably with the kind that the individual exercises himself, not the punitive variety'—with the attitudes of students and staff towards one another, and even with the organization of athletics and ethics of the playing-fields.

Dr Farnsworth has a clear awareness of the resistances that are likely to be met in trying to introduce such a scheme (or even a more modest one) and much of the book is designed to show why there is a need for a service of this kind.

No one would disagree, however, that maturity and mental health are desirable objectives for any group to seek. What the reader requires beyond that is an account of those mental health needs—if any—that are somehow peculiar and specific to a university. The book generally fails to provide this. With few exceptions—notably the chapter on 'The Development of College Mental Health Programs'—the discussions range too far outside the university's own precincts. Thus the individual conflicts and stresses which are described, with great sympathy and insight, are likely to be those of almost all adolescents in society whether students or not. And while the importance is acknowledged of the central occupational problems of learning and study, these are dealt with neither systematically nor deeply enough.

The book which is not primarily meant for psychiatrists but rather for 'educators' and parents, is very readable, not the least for the author's entertaining comments and observations—for example, on college fraternities and other student customs.

The Bibliography is well up to date and there is an appendix summarizing concisely the work of the international conference at Princeton in 1956, on student mental health.

JOHN HOPKINS

Mental Health in Home and School. World Federation for Mental Health. (Pp. 309. £2. 5s.) London: H. K. Lewis and Co. 1958.

These are the papers presented at the ninth

Annual Meeting of the World Federation for Mental Health, held in Berlin in August 1956. There are twenty-seven papers and several addresses. In all twenty-six nations took part.

As the title indicates, the accent is on the preservation or establishment of mental health in the child and the adolescent. A few papers, not the least interesting, are devoted to the problems of mental health in adults but on the whole it is the younger generation which is the general pre-occupation.

It is difficult to select any particular paper without doing injustice to others. The reviewer was particularly interested in a paper by Dr David M. Levy (U.S.A.) on 'Maternal Feelings Towards the Newborn', a remarkable paper on the 'Education of the Deaf Mute Child' by Dr J. P. de Reynier (Switzerland), a paper by Prof. Dr Syuzo Naka (Japan) on the 'Awaking Drug Addiction among Japanese Children and Young People', and a paper by Dr Tigani El Mahi (Sudan) which, in only two and a half pages, manages to convey in the most vivid and fascinating way the sort of problems the psychiatrist will encounter in that country.

Not all the papers have the same immediate scientific, sociological or anthropological appeal of the three papers just mentioned, but as one reads on and follows the discussion reports, one is nevertheless gradually caught up by something which is rather difficult to describe but which constitutes the real interest and significance of a meeting of this kind, namely, what one might call its international significance. Dr Brock Chisholm of Canada in his paper on 'Mental Health and Public Affairs' talked of the need to abandon National strivings and work toward a concept of mental health which would include in its definition awareness of one's responsibility as a citizen of the world. He is the only speaker to have made this point explicitly and one cannot help feeling that these are the words of leadership one expects to hear from a World Federation for Mental Health. Even without going quite as far as Dr Chisholm, one can certainly see hope in the way that the world as a whole is becoming so much more responsible in its tasks in the field of mental health, and so much more determined to treat it as a world problem, not as something limited to national boundaries.

P. M. PLOYÉ

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288. 23 October 1957. Miss NORAH GIBBS: Problems of Development in Physically Handicapped Children.
289. 27 November 1957. Prof. M. POLANYI: The Structure of the Modern World.
290. 11 December 1957. Miss ANNA FREUD: Psychoanalysis and the Observation of Behaviour.
291. 22 January 1958. Dr D. RUSSELL DAVIS: Clinical Problems and Experimental Researches (Address from the Chair).
292. 26 February 1958. Dr E. J. ANTHONY: Observations on Psychotic Children.
293. 26 March 1958. Dr J. CULLEN: The Breakdown of Perceptual Skills in Neurosis and Psychosis.
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